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MEMOIRS
OF THE
Royal Society;

Or, a New ABRIDGMENT of the

Philosophical Transactions.

Giving an ACCOUNT of the Undertakings, Studies, and Labours of the LEARNED and INGENIOUS in many considerable Parts of the World; from the first Institution of that ILLUSTRIOUS SOCIETY in 1665, to 1740.

In the Course of this WORK, every Thing is carefully extracted from the ORIGINALS, according to the Order of Time, the LATIN TRACTS Englished; the Terms of ART explained; the Theoretical PARTS applied to Practice; and the whole Illustrated with a great Number of COPPER PLATES.

A PERFORMANCE of general Use for the Knowledge and Improvement of MATHEMATICKS, NATURAL PHILOSOPHY, TRADES, MANUFACTURES, ARTS, &c.

By Mr. BADDAM.

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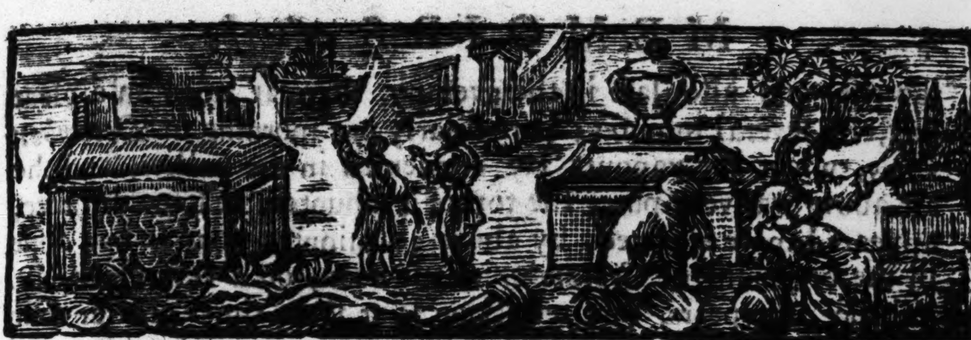

MEMOIRS
OF THE
ROYAL SOCIETY;

Being a New ABRIDGMENT of the

PHILOSOPHICAL TRANSACTIONS.

Observations on the height of the Barometer, *at different Elevations above the Surface of the* Earth; *by* Dr. Nettleton. Phil. Transf. N° 388. p. 308.

DR. *Nettleton*, having meafured a hill of a confiderable height in a clear day, and obferved the mercury at the bottom and top, found, according to that eftimation, that about 90 feet or upwards, were requifite to make the mercury fall one tenth of an inch; but coming afterwards to repeat the experiment on a cloudy day, when the air was fomewhat grofs and hazy, the fame angles were fo much increafed by refraction, as to make the hill much higher than before, tho' they were carefully taken with good inftruments, both at that time and before.

The Dr. afterwards frequently obferved at home, by pointing the quadrant to the tops of fome of the neighbouring mountains, that they would appear higher in the morning before fun-

ſun-riſe, and alſo late in the evening, than at noon in a clear day, by ſeveral minutes; particularly one morning in *December* 1725, when the vapours lay condenſed in the vallies, and the air above was very pure, the top of a mountain at ſome diſtance, appeared more elevated, by above 30 minutes, than it had done in the beginning of *September* about noon, on a very clear day. Whence it appears, that refraction is at ſome times greater than at others; but probably it is at all times very conſiderable; and as there is no certain rule to make allowance for it, it ſeems likely, that all obſervations made on very high hills, eſpecially when view'd at a diſtance and under ſmall angles, as they commonly are, are uncertain and ſcarce to be depended on, generally erring in making the heights greater than they really are.

The Dr. then proceeded to obſerve, as near as poſſible, the alteration of the mercury in ſome ſmaller perpendicular elevations, he could meaſure with a line; and alſo on the tops of ſome hills of a moderate height, whoſe altitude he could obſerve moſt commodiouſly, and by taking the angles large, avoid the danger of any conſiderable refraction.

At the bottom of the tower of *Halifax* church, the mercury ſtood at 29 inches 78 tenths. At the top it ſubſided to 29.66. The height of the place, where the obſervation was made, was found to be 102 feet.

At the bottom of a coal-mine, near *Halifax*, the mercury ſtood at 29.48. At the top it fell to 29.32. The depth of the mine being meaſured was found to be 140 feet.

At the bottom of another mine, the mercury was obſerved to ſtand at 29.50. At the top it fell to 29.23. The depth of this mine was 236 feet.

At the foot of a ſmall hill, whoſe height could be meaſured very exactly, the mercury ſtood at 29.81. At the top it fell to 29.45. The height of the hill was 312 feet.

At the bottom of *Halifax* hill, commonly called the *bank*, the mercury was obſerved to ſtand at 30. At the top it fell to 29.41. The height of this hill was found to be 507 feet.

Mathematicians demonſtrate, that the denſity of the air decreaſes in a geometrical progreſſion, as the elevation increaſes in an arithmetical one; and conſequently, that the logarithms of the denſities are, as the elevations reciprocally. But the weight of the air being as its denſity; and the height of the mercury in the barometer being always proportional to the weight of the air, it follows, that the logarithms of the heights of the mercury

cury are reciprocally, as the elevations: Whence having found by observation, what elevation is required to make the mercury stand at any given height, it will be easy to determine, how much is requisite to reduce it to any other height proposed. If we make 30 inches the standard height of the mercury, equal to unity, and suppose an elevation of 85 feet be required to make it fall one tenth of an inch from that height, as by these observations it is very nearly; then as the logarithm of $\frac{30,0}{29,9}$ is to 85, so is the logarithm $\frac{30,0}{29,5}$ to the number of feet required to make it fall half an inch, and so of the rest. When the mercury stands above 30 inches, the numbers will be negative, and shew the descending spaces; by which method the Dr. computed the following tables.

The latter table, which contains the differences of the numbers in the former was of very great use to the Dr. when in these experiments, the mercury stood at any other height in the tube, besides 30 inches, and fell any number of tenths or parts of a tenth, by adding the numbers corresponding thereto, or proportional parts of them, to find the elevation required in the table, to make the mercury fall so much; and thereby readily to compare with it the heights found by observation. And tho' some small errors, in the observations, make them vary a little from each other; yet in the main they agree as near as possible with the numbers of the table; as did also several other experiments: Whence those numbers seem not far from the truth.

That the air is colder, as well as lighter and rarer, in places situated high, than in vallies and low grounds, is generally known; and in order to learn, how much it might be so, the Dr. got a friend of his, who lived higher than he did, to observe the portable barometer and thermometer at his house for some days, placed as near as possible in the same circumstances with the Dr's; and for 20 days his barometer was at a medium found to stand three tenths, and the thermometer three degrees two tenths lower than the Dr's; allowing for the difference of the instruments, which had been observed before.

At another place the barometer, at a medium for 14 days, stood lower by 4.46 and the thermometer by 4 deg. 4. At another place which was very high upon the moors, at a medium for 10 days, the barometer stood lower by .65, and the thermometer fell 7 deg.

A Table

A table shewing the number of feet ascending, required to make the mercury fall to any given height in the tube, from 30 to 26 inches. As also the number of feet descending, required to make the mercury rise, from 30 to 31 inches.

Inches	Dec.	Feet	Dec.	Inches	Dec.	Feet	Dec.
31	0	834	79	27	9	1847	55
30	9	752	53	27	8	1938	97
30	8	670	01	27	7	2030	72
30	7	587	21	27	6	2122	80
30	6	504	15	27	5	2215	21
30	5	420	82	27	4	2307	95
30	4	337	21	27	3	2401	02
30	3	253	32	27	2	2494	44
30	2	169	10	27	1	2588	20
30	1	84	72	27	0	2682	33
30	0	00	00	26	9	2776	80
29	9	85	00	26	8	2871	62
29	8	170	29	26	7	2966	79
29	7	255	87	26	6	3062	32
29	6	341	73	26	5	3158	21
29	5	427	89	26	4	3254	46
29	4	514	34	26	3	3351	07
29	3	601	08	26	2	3448	05
29	2	688	11	26	1	3545	41
29	1	775	44	26	0	3643	14
29	0	863	08				
28	9	951	01				
28	8	1039	25				
28	7	1127	80				
28	6	1216	66				
28	5	1305	83				
28	4	1395	32				
28	3	1485	13				
28	2	1575	26				
28	1	1665	70				
28	0	1756	47				

A table

A table shewing the number of feet required to make the mercury fall one tenth of an inch from any given height in the tube, from 31 to 26 inches.

Inches	Dec.	Feet	Dec.	Inches	Dec.	Feet	Dec.
31	0	82	26	27	9	91	42
30	9	82	53	27	8	91	75
30	8	82	79	27	7	92	08
30	7	83	06	27	6	92	41
30	6	83	33	27	5	92	74
30	5	83	61	27	4	93	07
30	4	83	89	27	3	93	41
30	3	84	16	27	2	93	76
30	2	84	44	27	1	94	12
30	1	84	72	27	0	94	47
30	0	85	00	26	9	94	82
29	9	85	29	26	8	95	17
29	8	85	58	26	7	95	53
29	7	85	86	26	6	95	89
29	6	86	16	26	5	96	25
29	5	86	45	26	4	96	61
29	4	86	74	26	3	96	98
29	3	87	03	26	2	97	36
29	2	87	33	26	1	97	73
29	1	87	63	26	0	98	10
29	0	87	93				
28	9	88	24				
28	8	88	55				
28	7	88	86				
28	6	89	17				
28	5	89	49				
28	4	89	81				
28	3	90	13				
28	2	90	45				
28	1	90	76				
28	0	91	09				

A Baro-

A Barometrical *Experiment; by M.* And. Celsius. Phil. Transf. N° 388. p. 313. *Translated from the* Latin.

FOR observing the variation of the column of mercury in the barometer, according to the different heights of the atmosphere, the deep mines in *Swedland* may be reckoned peculiarly adapted: For, not only their depth may be measured with all possible accuracy, but likewise the whole observation perform'd in a short time; which advantage such frequently want, as would make the like experiments on high mountains: If, therefore, a great many experiments were made in different mines, no doubt, but the true progression, by which the density of the air decreases, would at length be discovered.

M. *George Vallerius* had long before observ'd the ascent of the mercury in the great *Kupferberg* mine; vide *Mem. de l'Acad. Roy. des Sc. l'Ann.* 1712. p. 108.

Aug. 28. 1724. M. *Celsius* made the following experiment in the *Salan* silver mine, about seven miles to the west of *Upsal*, at the entry, to the Drottning Christinae Schatcht, or Queen *Christina*'s shaft, he observ'd the height of the mercury at 30 inches 38 centesimal parts or the $\frac{3038}{1200}$ parts of a *Swedish* foot; he was then let down with the barometer in a vessel by a rope, to the depth of 636 foot, where he observ'd the mercury ascend 30 inches 98 centesimal parts; from thence being drawn up again to the mouth of the shaft, he observ'd the column of mercury at the same height as before, *viz.* 30 inches 38 centesimal parts: So that mercury rais'd to the height of 636 feet in the air, falls 6 lines or $\frac{6}{120}$ parts of a foot; and consequently, if the air were suppos'd of equal density every where, the variation of one line in the column of mercury would answer to 106 feet perpendicular height. During the time of the observation, there was a little rain and wind; yet no sensible alteration could be observ'd at the same time in the column of mercury in another barometer, fix'd to a wall above the mine.

Next day, the sky serene and calm, the mercury stood at 30 inches 36 centesimal parts at the foot of the church of *Sale*, not far from the mine; but going up 145 foot high in the tower of the said church, he found the mercury at 30 inches 23 centesimal parts: So that the height of 111 foot and $\frac{7}{13}$ parts answers to the descent of one line in the barometer; the foot of the church is almost 60 feet below the superfices of the mine.

That this observation may be duly compar'd with the experiments of this kind, made by others, it is to be noted, that the

the ratio between a *Swedish* and *Paris* royal foot, is as 1000 to 1096, or 125 to 137 nearly; which M. *Celsius* very accurately observ'd by comparing a *French* foot made of brass by the ingenious artist *Chapotot*, with the *Stiernhielm* foot, kept in the public library at *Upsal.*

Remarks on the Observations made on Sir Isaac Newton's Chronological Index, *translated into* French *by the Observator, and publish'd at* Paris; *by Sir* Isaac Newton. Phil. Trans. N° 389. p. 315.

NOV. 11. 1725. Sir *Isaac Newton* had a small tract in print (deliver'd to him as a present, from M. *Cavelier* jun. a bookseller at *Paris*, a person unknown to him) entitul'd *Abregé de chronologie de M. le Chevalier Newton, fait par lui meme, et traduit sur le manuscript Anglois*; to which the bookseller hath prefix'd an advertisement, in which he endeavours to defend himself for printing it without Sir *Isaac Newton*'s leave, saying, that he had writ three letters to Sir *Isaac* for his leave, and in the third had told him, that he would take his silence for a consent; and that he had charged one of his friends at *London* to speak to Sir *Isaac*, and procure his express answer; and that having long expected Sir *Isaac*'s answer, he thought he might take his silence for a consent; and so procur'd a privilege and printed it, and then receiv'd Sir *Isaac Newton*'s answer from his friend, which was as follows.

'I remember that I wrote a chronological index for a particular friend, on condition that it should not be communicated. As I have not seen the manuscript which you have under my name, I know not whether it be the same. That which I wrote was not at all done with design to publish it. I intend not to meddle with that which hath been given you under my name, nor to give my consent to the publishing it'. I am

Your very humble Servant,

London May 27.
1725. O. S.

ISAAC NEWTON.

The privilege was granted *May* 21. and registered *May* 25. O. S. Sir *Isaac Newton*'s letter was dated *May* 27. and the chronological index or abridgment, as it is call'd, was printed before the arrival of Sir *Isaac Newton*'s letter, and

kept ever ſince in order to be publiſh'd at a convenient time. The bookſeller knew, that Sir *Iſaac Newton* had not ſeen the tranſlation of the abridgment, and without ſeeing it, could not in reaſon give his conſent to the impreſſion. He knew that the tranſlator was unknown to Sir *Iſaac Newton* and againſt him; and therefore, he knew, that it was not fit, that Sir *Iſaac* ſhould give his conſent, nor be aſk'd to do it. He knew, that the tranſlator had written a confutation of the paper tranſlated; and that this confutation under the title of obſervations, was to be printed at the end of it; and the bookſeller told Sir *Iſaac Newton* nothing of all this, nor ſo much as the name of the obſervator; and yet aſk'd his conſent to the publication.

After the recital of Sir *Iſaac Newton*'s letter, he adds, that the author of the tranſlation and obſervations upon it, pretends to have an entire certainty, that this index or abridgment of chronology, is the ſame with the writing, own'd by Sir *Iſaac Newton* in his letter, and is perſuaded, that the manuſcript which had been communicated to him, had been copied from that of the particular friend, mentioned in Sir *Iſaac Newton*'s letter: And therefore, the manuſcript which had been communicated to the bookſeller, is that of Abbé *Conti*; who, being about ſeven years ago in *England*, gave Sir *Iſaac Newton* notice, that the friend abovementioned deſired to ſpeak with him, and this friend then deſired a copy of what he had written about chronology; Sir *Iſaac* replied that it was imperfect and confus'd; but in a few days would draw up an abſtract thereof, if it might be kept ſecret. And ſome time after he had done this and preſented it, this friend deſired that A. *Conti* might have a copy of it. He was the only perſon that had a copy, and he knew that it was a ſecret; and that it was at the deſire of this friend, and by Sir *Iſaac Newton*'s leave, that he had a copy, and he kept it ſecret, while he ſtaid in *England*; and yet, without this friend's leave or Sir *Iſaac Newton*'s, he diſpers'd copies of it in *France*, and got an antiquary to tranſlate it into *French* and to confute it; and the antiquary got a printer to print the tranſlation and the confutation; and the printer hath endeavoured to get Sir *Iſaac Newton*'s leave to print the tranſlation, without ſending him a copy thereof to be peruſed, or telling him the name of the tranſlator, or letting him know, that his deſign was to print it with a confutation, unanſwer'd and unknown to him,

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The tranſlator near the end of his obſervations p. 90. ſays, 'I believe I have ſaid enough concerning the *epocha* of the '*Argonauts* and the length of generations, to make people 'cautious about the reſt: For, theſe are the two foundations 'of all this new ſyſtem of chronology'. What he ſays concerning the *epocha* of the *Argonauts*, is founded on the ſuppoſition, that Sir *Iſaac Newton* places the equinox in the time of the *Argonautic* expedition, 15 degrees from the firſt ſtar of *Aries*, p. 75, 79. Sir *Iſaac Newton* places it in the middle of the conſtellation, and the middle is not 15 degrees from the firſt ſtar of *Aries*. The obſervator grants, that the conſtellations were form'd by *Chiron*, p. 70, 71, 79, and that the ſolſtices and equinoxes were then in the middle of the conſtellations, p. 65, 69, 75. And that *Eudoxus*, in his *Enoptron* or *ſpeculum* cited by *Hipparchus*, follow'd this opinion, p. 62, 63, 65, 69, 79. And *Hipparchus*, (vide *Hipparchus* publiſh'd by *Petavius* Vol. III. p. 116, 117, 119, 120.) names the ſtars, thro' which the colures paſs'd in this old ſphere, according to *Eudoxus*, and ſays expreſsly, that *Eudoxus* drew one of theſe colures thro' the middle of *Cancer* and the middle of *Capricorn*; and the other thro' the middle of *Chelæ* and the back of *Aries*; and the colure paſſing thro' the back of *Aries*, paſſes thro' the middle of *Aries*, and is but eight degrees from the firſt ſtar of *Aries*. Sir *Iſaac Newton* follows *Eudoxus*, and by ſo doing, places the equinoctial colure about 7 degrees 36 minutes from the firſt ſtar of *Aries*. But the obſervator repreſents, that Sir *Iſaac Newton* places it 15 degrees from the firſt ſtar of *Aries*; and thence deduces, that he ſhould have made the *Argonautic* expedition 532 years earlier than he does. Let the obſervator rectify his miſtake, and the *Argonautic* expedition will be where Sir *Iſaac Newton* places it.

As to the length of generations, the obſervator ſays, that Sir *Iſaac Newton* reckons them one with another at 18 or 20 years a-piece p. 52, 55; which is another miſtake. Sir *Iſaac Newton* agrees with the ancients in reckoning three generations, at about 100 years. But the reigns of kings he does not equal to generations, as the ancient *Greeks* and *Egyptians* did; but he reckons them only at about 18 or 20 years a-piece one with another, when 10 or 12 kings, or more, are taken in one continued ſucceſſion. Thus, the firſt 24 kings of *France* (*Pharamond*, &c.) reign'd 458 years, which is, one with another, 19 years a-piece. The next 24 kings of *France*

(*Ludovicus Balteus*, &c.) reign'd 451 years, which is one with another, 18 ¾ years a-piece. The next 15 kings (*Philippus Valesius*, &c.) reigned 315 years, which is one with another, 21 years a-piece. And all the 63 kings of *France* reign'd 1224 years, which is 19 ½ years a-piece. And if the long reign of *Lewis* XIV. be added, the 64 kings of *France* will reign but 20 years a piece. And they that examine the matter, will find it so in other kingdoms. And Sir *Isaac Newton* shortens the duration of the ancient kingdoms of *Greece*, in the same proportion he shortens the reigns of their kings; and thereby places the *Argonautic* expedition about 44 years, and the taking of *Troy* about 76 years after the death of *Solomon*, and finds *Sesostris* cotemporary to *Sesac*.

So that the observator has mistaken Sir *Isaac Newton*'s meaning, in the two main arguments on which the whole is founded, and hath undertaken to translate and confute a paper he did not understand, and been zealous to print it without his consent; tho' he thought it good for nothing, but to get himself a little credit, by translating it to be confuted, and confuting his own translation.

The observator says, that Sir *Isaac Newton* supposes, that the *Egyptians* began, about 900 years before *Christ*, to form their religion, and deify men for their inventing of arts, notwithstanding that it appears by the scriptures, that their idolatry and arts were as old, as the days of *Moses* and *Jacob*, p. 82, 83. But he is again mistaken. Sir *Isaac Newton* does not deny, that the kingdom of the *Lower Egypt*, call'd *Mizraim*, had a religion of their own, till they were invaded and subdu'd by the shepherds, who were of another religion. But Sir *Isaac Newton* says, that when the *Thebans* expell'd the shepherds, they set up the worship of their own kings and princes. He also says, that arts were brought into *Europe* principally by the *Phœnicians* and *Curetes*, in the time of *Cadmus* and *David*, about 1041 years before *Christ*; and does not deny, that they were in *Phœnicia*, *Egypt* and *Idumea* before they were brought into *Europe*.

The observator likewise says, that 884 years before *Christ*, Sir *Isaac Newton* places the beginning of the canicular cycle of the *Egyptians* upon the vernal equinox, tho' that cycle never begins in spring, p. 84, 85. But he is again mistaken, Sir *Isaac Newton* does not meddle with that cycle, but speaks of the *Egyptian* year of 365 days.

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The obſervator repreſents, that Sir *Iſaac Newton* has a great work to come out. But Sir *Iſaac* never told him ſo.

Abbé *Conti* came into *England* in ſpring 1715; and while he ſtaid in *England*, he pretended to be Sir *Iſaac Newton*'s friend; but aſſiſted M. *Leibnitz* in engaging the former in new diſputes, and hath ſince acted in the ſame manner in *France*. The part he acted in *England* may be underſtood by the character given of him in the *Acta Erudit.* for the year 1721. p. 90. where the editor, excuſing himſelf from repeating ſome diſputes, which had been publiſh'd in thoſe *Acta*, ſubjoins: 'Let it, therefore, ſuffice to obſerve, that 'when the Abbé *Conti*, a noble *Venetian* (of whom M. *Leibnitz* 'acknowledges M. *Herman* gave a good character) came over 'from *France* into *England*, he undertook to be mediator in 'the diſputes between Sir *Iſaac Newton* and M. *Leibnitz*; 'and took care to tranſmit the letters of the one to the other.' And how M. *Leibnitz* by the Abbé's mediation, endeavoured to engage Sir *Iſaac Newton*, againſt his inclination, in new diſputes, about occult qualities, univerſal gravity, the *Senſorium* of God, ſpace, time, *vacuum*, atoms, the perfection of the world, ſupramundane intelligence, and mathematical problems, is mentioned in the preface to the ſecond edition of the *Commercium epiſtolicum*. And what the Abbé hath been doing in *Italy*, may be underſtood by the diſputes, rais'd there by one of his friends, who denies ſeveral of Sir *Iſaac Newton*'s optical experiments, tho' they have been all tried in *France* with ſucceſs. But Sir *Iſaac Newton* hopes, that theſe things and the perpetual motion will be the laſt efforts of this kind.

Of Camphire; *by M.* Charles Neuman. Phil. Tranſ. N° 389 p. 321. *Tranſlated from the* Latin.

IT is laid down in the treatiſes, publiſh'd on camphire, that (beſides the common ſort, which may be got in pretty large quantity) another ſort may likewiſe be prepar'd from cinnamon-root, zedoary of *Ceylon*, camphorated rosemary, ſouthernwood and other aromatics; and this cannot be denied in ſome reſpect, and is confirm'd by M. *Neuman*'s own obſervation: Yet nevertheleſs he owns, that in all the places he travell'd, he never ſaw (tho' he enquired diligently about it) any ſuch, nay, not the leaſt quantity, prepar'd from the above-mentioned or any other vegetables, that agree'd with oriental camphire in its ſolid and dry conſiſtence; but whatever

ever of this sort was shewn him at *Amsterdam* was that oil, which is said to be distill'd in the *Indies* from cinnamon-root, and smells of camphire, as well as cinnamon; tho' he still doubt, whether it be a genuine distillation, or the common oil of cinnamon, impregnated with a large quantity of camphire: Since he knows from experience, that commonly several commodities, especially such as are for medical uses, are counterfeited, and adulterated, particularly in the abovementioned place: But however the case may be as to that oil, even supposing, that there were no adulteration, and that it might be shewn, that camphire, even of the same solid consistence, may be prepar'd from cinnamon-root, from *Ceylon* bezoar and other oriental vegetables; yet none as far as M. *Neuman* knows, ever produced any thing (of the like consistence with camphire) from *European* vegetables; tho' several have asserted a great many things about rosemary, and likewise given their conjectures about some other of our own native plants; even tho' M. *Hofman* and Mr. *Boyle*, have respectively given an account, the former of his obtaining something like it from the flowers of roses and aloes, and the latter from annis-seed oil, vide *Hofman Dissert. de Camphora & Obs. Physico-Chem.* and *Boyle*'s *Tr. de firmitate & fluiditate*: Yet M. *Neuman* is entirely persuaded, that whatever was produced from these three things, that resembled camphire, was soft and unctuous, or at most of a consistence like butter, but not at all of a dry, crystalline, brittle and hard consistence like camphire, which may be easily gather'd from their own descriptions of it: Not to mention what is commonly known, *viz.* that annis-seed oil, when expos'd to the cold air, is condens'd like butter, but not that it thereby becomes camphire. Besides what we find mentioned in books, some, and among those an apothecary, otherwise well skill'd in his business, told M. *Neuman*, that they could prepare camphire from other *European* plants, which yet he never had the good fortune to see; however, M. *Neuman* readily believes, they might as easily obtain their camphire, as he did his.

From common thyme M. *Neuman* obtain'd a true, dense, crystalliform, camphire, agreeing with the oriental in all its qualities, only that it had a different smell, and could bear all the trials, hitherto made, and that possibly could be devised.

In 1719 M. *Neuman* distill'd a pretty large quantity of oil from the said plant, and separating it with cotton from the water,

water, he obſerv'd ſmall figur'd cryſtals adhering both to the mouth of the glaſs and to the cotton, impregnated with the oil, that more and more retarded the diſtillation, otherwiſe quick enough; ſo that he was oblig'd (contrary to the uſual method,) ſometimes to renew the cotton. He was ſurpriz'd at this uncommon and extraordinary accident, and after various thoughts, he could not determine whence it happened, and what it was; he therefore, laid a bottle by, full of oil, and cover'd it well; and a few days after, he view'd the bottle, in order to obſerve, whether any cryſtals adher'd to the mouth of the glaſs; but he found none there, tho' he obſerv'd about the bottom a great many cryſtals, of different ſizes, ſome as big as filberds, and moſt of them of a cubical form, reſembling ſugar-candy. He did not ſuſpect it to be camphire, but rather a volatile ſalt, that conſidering the large quantity of the herb that was made uſe of, ſucceſſively mounted up to the top, and being diſſolv'd in the water, after this, it ſubſided, and the oil floating at top, did at length coaleſce in that manner. Pouring out, therefore, the oil, in order to obtain the ſalt, as he ſuppos'd it to be; he was at length very much ſurpriz'd, to find not a ſingle drop of water at the bottom; when he thought it adviſeable to free this ſaline *magma* or concretion from the bottom, and ſeparate it from the oil. In order to be aſſur'd about this ſalt, as he imagined it to be, he tried the experiment, by which ſalts of all ſorts and denominations are prov'd. He pour'd a large quantity of diſtill'd water upon the cryſtalliz'd matter, and agitating them a pretty while together, he let them ſtand for ſome hours; and every quarter of an hour he earneſtly expected the ſolution of the cryſtalliz'd matter; but in vain: For, he perceiv'd neither ſolution, nor any other change, only that the ſmaller particles mounted to the top, whilſt the larger ones, tho' well agitated together, always ſubſided, and the water was impregnated with the oily particles, that adhered to the cryſtalliz'd matter, but now freed by the agitation. He decanted the water, and pouring on ſome freſh, he again attempted the ſolution of the cryſtals, according to the nature of ſalts, after being freed from the oil, which, probably, hinder'd the ſolution of the cryſtalliz'd matter in the water, but they ſtill obſtinately reſiſted a ſolution, tho' ſeveral times attempted. Then quite laying aſide his notion of its being a ſaline concretion, he began to conjecture, that it muſt be ſomething elſe, *viz.* either a mix'd, oily, or reſinous volatile,

as

as it were, condensed from oil: Upon considering this, he examined several other subjects, to see if any of them could be compared with this; and on account of the resemblance, he immediately bethought himself of camphire. Having, therefore, procured some new subjects, in order to be assured of the matter, he accordingly was so: For, all the experiments he tried, exhibited the same phenomena, which common camphire does. Whence he certainly concluded, that this crystalliform body was no other than true camphire, which differs from the oriental, only in this, *viz.* that the former smells of thyme, and the latter of some other vegetable, which on account of its yielding a large quantity of camphire, has hitherto been called the camphire-tree.

From all this M. *Neuman* concluded, 1. That *camphora* or *caphura* is not a proper name, always expressing that sort of camphire only, that comes from the *Indies*; but rather an appellative (for instance, that of a salt, oil, water, spirit, *&c.*) So that to distinguish the species of camphire it has the name of the vegetable given it, from which it is prepared; as M. *Neuman*'s camphire may justly be called the camphire of thyme. 2. The controversy that has hitherto been carried on between authors about the species to which it belongs, does entirely cease; since it is not to be reckoned among resinous substances, volatile salts, oils or juices, far less among gums or bitumen's, as not agreeing in all respects with these; but sometimes in one and sometimes in another quality; partaking at one time more, and at another less of the principal quality, or perhaps a quite different one.

1. It cannot be called a resin, on account of what *Hofman* has observed, *viz.* 'all resins after burning leave ashes or some 'other earthy matter behind, whilst on the contrary camphire 'leaves nothing, but quite burns and evaporates. No resins 'included in a close vessel are entirely sublimed by fire, which 'yet is the case as to camphire: Resins, when distilled, yield a 'phlegm, oil, and in some measure a spirit, as it is called; but 'camphire none of these. There is no kind of resin, that like 'camphire can be dissolved, in spirit of nitre nor dissolved so 'soon, and in such quantity in spirits of wine, nor evaporated 'in warm water.'. 2. Much less can camphire be called a volatile salt, because it cannot be dissolved in water, nor so intimately incorporated therewith, as not to be observed, which invisible solution all salts must necessarily admit of, as is mentioned above. 3. Tho' camphire be a mix'd oily body, or condensed

denſed from oil, yet for that reaſon it cannot be called ſimply an oil, as other mixed ſolids, abounding with oil, are; inſomuch as this name is more eſpecially given to liquid ſubſtances or at moſt to ſuch as are thin and unctuous; but not to ſuch as are ſolid and dry; nor can any oil be ſublimed from a dry ſubſtance, as camphire. 4. Camphire cannot be called a juice, 'becauſe, as 'Hofman affirms, ſuch has obtained a mixture, that is either 'gummous, which is ſoon entirely diſſolved in water, or gummi-'reſinous, which is partly diſſolved in a watery, and partly, in 'an oleoſe, inflammable, ſpirituous, or ſome ſuch *menſtruum*; 'or reſinous, which latter are properly called reſins, of which 'there has been mention made under N° 1. none of whoſe pro-'perties agrees with camphire. For, ſhould any one call thoſe 'things juices, which partly are produced ſpontaneouſly, from 'trees and other vegetables, and partly prepared by art; 'tho' they be diſperſed and intermixed throughout the whole 'plant, its juice and other parts; yet what confuſion would 'it occaſion in deſcribing the eſſences of things? For, thus 'all difference of water, juice, gum, oil, balſam, reſin, and 'the like, would be quite at an end, and thoſe names of 'diſtinction would be of no manner of uſe.' 5. That camphire is neither a gum nor a bitumen, ſufficiently appears from what has been ſaid above; for, whatever is called a gum, entirely diſſolves in water, as alſo does the greateſt part of bitumen's; but neither admits of a ſublimation, as camphire does. Since there is nothing we know that can be compared with camphire, it is reaſonable, it ſhould have its own peculiar and ſpecific name by which, when any thing is called camphire, it may be immediately underſtood, that it is neither an oil, gum, volatile ſalt, reſin, ſpirit, or bitumen, but quite a different ſubſtance from all theſe, *viz.* a compoſition made up of ingredients, that conſtitute the true camphire, and of a very peculiar nature. Hence this queſtion, *viz.* whether camphire be a ſalt, gum, reſin, or juice, may be reckoned as prepoſterous, as if any ſhould aſk, whether a ſalt be an oil, or a gum be camphire.

3. From what has been ſaid, M. *Neuman* concludes, that all camphire, conſtantly retains its own ſpecific ſmell, and the oil of that plant, from which it is prepared; and that that ſmell cannot be ſeparated therefrom without deſtroying the whole camphire.

4. Hence it follows, that M. *Neuman*'s experiment of the camphire of thyme does abundantly confirm the formation of

the oriental camphire, namely, that it is obtained by diſtillation; in which there is ſcarce any other difference, only that there may be got a much greater quantity of camphire than oil from the camphire-tree, as it is called; when on the contrary the *European* vegetables yield a great deal of oil, but little camphire.

5. We may be perſuaded, that other *European* plants, abounding in a large quantity of eſſential oil, as well as thyme, will yield camphire, tho' M. *Neuman* found none hitherto, nor had a proper opportunity of making the trial.

6. Finally, in order diſtinctly to conceive the peculiar nature of camphire, and know wherein it conſiſts, M. *Neuman* gives the following definition of it, *viz.* that camphire, is a dry, white, pellucid, brittle, very ſtrong ſcented, ſubſtance, entirely evaporates not only in the fire, but likewiſe in a warm air, and on account of its mixture, conſiſts of a great deal of an inflammable, rarefied principle, a little phlegm, and a highly attenuated, or very ſubtile earth, intimately united together. 1. Camphire conſiſts of an inflammable rarefied principle, becauſe it not only takes fire ſoon and burns gently, even in water and ſnow, but likewiſe is very ſoon united with other ſubſtances, that copiouſly partake of the ſame principle, among which ſpirit of wine is very well known, and ſpirit of nitre deſerves to be farther conſidered, ſince very little is known about it. 2. Camphire contains ſome phlegm, ſince otherwiſe an inflammable principle, tho' in a conſiderable quantity, could not yield a flame. 3. That it contains a ſubtile earth no one will deny, becauſe without this, a dry cryſtalline body could not be form'd. 4. Nor will it be doubted, that this very ſubtile earth is intimately united with the other parts, even the minuteſt; if we conſider, that camphire when expoſed to the air only, if ſomewhat warm, evaporates, and leaves nothing behind.

This definition is confirmed by what has been ſaid already about the camphire of thyme, by conſidering, that it only proceeds from a ſubtile eſſential oil, in which there can be ſhewn no other than the three conſtituent parts abovementioned, *viz.* fire, water and earth, tho' with the following remarkable difference. 1. That the compoſition of camphire is more ſubtile than that of oil, nay, is the quinteſſence (ſo to ſpeak) and extract of oil. 2. Camphire contains only a little of the aqueous principle, but much more oil in compariſon of that. 3. The earth contained in camphire is ſo highly attenuated and ſubtile, as to evaporate ſpontaneouſly together with the other conſtituent parts;

parts; whereas on the contrary, the earth of oil is much groſſer, as may be obſerved in the rectification or exhalation of oil, where it appears at firſt like reſin or bitumen, and at length of a groſſer ſpecies.

Theſe, and other experiments, tried on camphire, made M. *Neuman* doubt, whether camphire can be obtained any other way than by diſtillation; particularly he ſuſpected the truth of what is ſaid about gathering the camphire in the iſland of *Borneo*; for, they ſay, that in that place camphire exſudates ſpontaneouſly like a gum or reſin from a tree; and ſo gathered by the inhabitants; of much greater value than the camphire of *Japan*, and for its ſcarcity ſo dear, that the inhabitants of that iſland give none of it to ſtrangers. But however the caſe be as to this tradition, M. *Neuman* has juſt cauſe to doubt of it; eſpecially, as volatility is eſſential to the very nature of camphire; for, ſhould it exſudate from trees, it would not bear ſo long the intenſe heat of the ſun, in that climate; but be ſoon evaporated, and that ſo much the more eaſily, becauſe in exſudating, it muſt be much more liquid; nor could it have the hard conſiſtence of camphire; and its liquidity would diſpoſe it to exhale ſo much the ſooner; ſince camphire, when once it is warmed, cannot be retained in a liquid conſiſtence, and as ſoon as it is melted, it immediately evaporates; on which account likewiſe it cannot be compared with reſin. M. *Neuman* is far from believing, that the native camphire (like the diſtilled kind, diſſolved, as it were, in its own diſtilled oil) flows from a tree, and afterwards is concreted in the manner of cryſtals upon the bark of the tree; ſince there are no experiments to prove, that nature produces eſſential, or other oils, as diſtilled by art; becauſe ſhe wants the proper *apparatus*: And he is almoſt apt to think, that the whole tradition about gathering camphire is taken from hear-ſay; and that the otherwiſe learned *Ten Rheine*, *Breynius*, *Herman* and others, tranſcribed it from each other; but that none, ſo far as is credible, ever ſaw it with their own eyes upon the tree, which is not the firſt miſtake that has been committed in natural hiſtory. In fine; hence M. *Neuman* concludes, that this ſpecies of camphire is ſo very rare, that none ever ſaw it, and, probably, never will.

Obſervations on the Dipping Needle, *made at* London *in the beginning of the Year* 1723; *by Mr.* George Graham, Phil. Tranſ. N° 389. p. 332.

ABOUT the time Mr. *Graham* was obſerving the variation of the horizontal needle, he likewiſe made ſome experiments with the dipping-needle, in order to try, if the dip and vibrations were conſtant and regular. The needle he made for this purpoſe was 12 inches and one tenth long, ½ an inch broad in the middle, but not above 1 tenth near the extremities, which were filed to fine edges, and it was about ⅓ of a tenth thick. The ends of the axis, upon which the needle turned, were very ſmooth, and no bigger than was neceſſary for the ſupport of the needle, which weighed 9 penny weight 21 grains, or about ½ an ounce *Troy*. The ends of the axis were placed upon the edges of 2 thin plates of ſteel, that were hard and well poliſhed, and parallel to the horizon, that the needle, when vibrating, might roll, and not ſlide upon the edges of the plates, to avoid the friction they would have been liable to, by moving in holes. He provided a braſs ſemi-circle, graduated from the lowest point each way; and a few of the degrees about that part of the ſemicircle, that anſwered to the dip, were divided into 6 equal parts. By the help of ſcrews the ſemicircle could be brought to a due ſituation; and by 2 ſight levels, placed at right angles to each other, any change of ſituation was eaſily perceived, and by the ſcrews it could be readily reſtored to its former poſition: All was incloſed with glaſs, to ſecure the needle from being diſturbed by the motion of the air. Here Mr. *Graham* takes notice of the great difficulty there is of poiſing the needle ſo exactly, before it is touched with the loadſtone, as to take any poſition indifferently: For, when it is pretty near the truth, it is exceeding troubleſome to place it at reſt in the poſition deſired, in order to try which way it is inclined to move. It cannot be done in the open air; for, the leaſt motion thereof will diſturb the needle; and when it is ſhut up, it is no eaſy matter to ſettle it in the place intended. And that there will be a ſenſible difference of the dip, upon ſhifting the ſides of the needle, whatever pains be taken to prevent it, Mr. *Graham* was fully ſatisfied from the following experiments.

Experiment 1. *March* 20, 1722. He touched both ſides of that end of the needle, he intended ſhould point ſouth, upon the north pole of a ſmall *terrella*; after which he cauſed it to vibrate

vibrate in an arch of 10 degrees, and reckoned the time by a pendulum clock, ſhewing ſeconds, till the needle had performed 50 vibrations.

	′	″
It performed the firſt 25 vibrations in	2	58
The next 25 vibrations in	2	27
The 50 in	5	25
Which gives for each vibration at a medium		6,5
The needle dipped 73° 15′		

Exp. 2. Then he ſhifted the needle ſo that the ſide, which before reſpected the eaſt, was now turned weſt, and cauſing it to vibrate in the ſame arch, as before, it performed

	′	″
The firſt 25 vibrations in	2	49
The next 25 in	2	39
The 50 vibrations in	5	28
That is, each vibration in		6,56
The dip 73° 50′		

Exp. 3. He now touched the ſame end of the needle, a ſecond time, on both ſides, upon the ſame ſtone, and ſuffering it to vibrate as before.

	′	″
It performed 25 vibrations in	2	49
That is, one vibration in		6,76
The dip 73° 20′		

Exp. 4. The needle was now ſhifted, and ſtood as in the ſecond experiment.

	′	″
It performed 25 vibrations in	2	41
That is, one vibration in		6,44
Dip	73°	45′

Exp. 5. The ſame end of the needle being now touched twice on each ſide, with the loadſtone, preſented by the Lord *Paiſley* to the *Royal Society*, in the armour,

It

			′	″
It performed the first 25 vibrations in			1	58
The next 25 in			1	46
The 50 vibrations in			3	44
That is, each vibration in				4,48
The dip	73°	55′		

Exp. 6. The needle being turned, and standing as in the second and fourth experiments, it performed

			′	″
The first 25 vibrations in			2	00
The next in			1	57
The 50 vibrations in			3	57
That is, each vibration in				4,74
The dip	74°	10′		

Exp. 7. He now touched the needle at both ends with the same stone, with which it was touched in the fifth experiment, after which it performed

			′	″
The first 25 vibrations in			1	35
The next 25 in			1	34
The 50 in			3	9
That is, each vibration in				3,78
The dip	74°	20′		
The dip repeated with the needle taken off and replaced	74	20+		

Exp. 8. Upon shifting the needle, it performed

			′	″
The first 25 vibrations in			1	33
The next 25 in			1	34
The 50 in			3	7
The dip	74°	25′		
The dip repeated	74	03		

N. B. The needle had the same side to the east in the 1, 3, 5, and 7th experiments; and had that side turned westward in the 2, 4, 6, and 8th; and Mr. *Graham* began to count the vibrations, when he observed it to vibrate just 10 degrees, as near as

as he could conjecture. All these experiments were made with sufficient care in every particular, excepting the quantity of the dip, which requires the divisions of the semicircle to be very equal, aud the 90th degree to be perpendicular under the axis of the needle; this last he found was a little faulty, the dip being in reality greater than the semicircle shewed it. After he had rectified this error, and new touched the needle, upon that part of the armour to which iron is applied, when it is to be lifted by the stone, it performed the same number of vibrations in less time than in any of the former trials. He now determined to observe, for some considerable time, both the dip and vibrations, without fresh touching the needle.

It appears by the following observations, that there is a very considerable difference, both in the quantity of the dip, and in the quickness of the vibrations.

N. B. In all these experiments, the needle was placed, so as to vibrate exactly in the plane of the magnetic meridian; and sufficiently distant from all iron that could affect it, as far as he could perceive, till he had occasion to put up a very large iron rod in the room above it, which immediately altered the dip of the needle, and thereby put an end to these trials.

1723.		Dip		Time of the day.	
		° ′		h. ′	
March	29.	75=00	at	10=00.	
		74=53		4=15	
	30.	74=55+		1=00	
		74=50—		4=00	
	31.	74=50—		10=00	
		74=50—		12=30	
		74=50—		2=15	
April	1.	74=25		6=45	
		74=25—		7=15	
		74=20+		9=00	
	2.	74=20+		7=30	A. M.
		74=20+		7=30	P. M.
	3.	74=20+		9=30	
		74=20+		12=30	
		74=50		4=15	

Dip

	Dip	Time of the day.
	° ′	h. ′
April 4.	74=55 +	10=00
	74=50 +	11=15
	74=40	12=45
	74=35	7=30
5.	74=40	9=15
	74=40	1=45
	74=40 +	5=30
	74=30 +	8=15
6.	74=35	10=00
	74=35	12=00
7.	74=35 +	10=20
	74=35 +	12=30
	74=35	4=00
	74=35	6=30
8.	74=40—	12=15
	74=40—	3=30
April 9.	74=40—	10=00
	74=40—	4=15
10.	74=40—	10=00
	74=30 +	8=00
11.	74=35 +	10=00. A. M.
13.	74=40—	
14.	74=40—	10=45
	74=40—	11=15
	74=35	5=10
	74=35	8=17
15.	74=35	9=10. A. M.
16.	74=35	11=00
	74=30 +	8=45
17.	74=45	12=25
18.	74=40 +	9=15
	74=45	5=00
19.	74=45	9=00. A. M.
20.	74=45	
21.	74=50	10=30

	Dip	Time of the day.
	° ′	h. ′
April 23.	74=50	12=00
26.	74=50+	2=30
27.	74=55	
28.	74=00	1=00. P. M.
	75=00	3=15
	74= 8	5=15
30.	74=40	3=15
May 1.	74=45	1=30
2.	74=45	12=00
	74=45+	1=00
	74=40+	3=50

The weight of the dipping needle 9 pwt. 21 gr. *Troy*.

N. B. The mark + ſignifies ſomething more than is here ſet down, and — ſignifies ſomething leſs; but the difference could ſcarce amount to more than two minutes.

Experiments of the vibrations of the dipping needle, beginning with an arch of 10 degrees, with the times in which 100 vibrations were performed.

1723 h ′
April 1. about 7=15 afternoon.
′ ″
Firſt 50 in 3= 2
Laſt 50 in 2=45

The 100 in 5=47. dip 74°=25′—

April 2. in the evening.
Firſt 50 in 3= 3
Laſt 50 in 2=43

The 100 in 5=46. dip 73=20+

April 3. about 4 in the afternoon.
Firſt 50 in 2=52
Laſt 50 in 2=39

The 100 in 5=28. dip 74=50+

1723
Repeated about an hour after.
First 50 in 2′=53″
Last 50 in 2=35

The 100 in 5=28. dip 74=50+

April 4. about 11′ 15[h] in the morning
First 50 in 2=54
Last 50 in 2=30

The 100 in 5=24. dip 74=50+

April 28. about 5=15 afternoon.
First 50 in 2=48
Last 50 in 2=16

The 100 in 5= 4. dip 74=58

Repeated

First 50 in 2=47
Last 50 in 2=16

The 100 in 5= 3. dip 74=58

May 20.
First 50 in 3′=11″
Last 50 in 3= 1

The 100 in 6=12

Repeated, the needle being new touched.
First 50 in 2=38
Last 50 in 2=23

The 100 in 5= 1. dip 74=35

Repeated again about an hour after.

First 50 in 2′=38″
Last 50 in 2=20

The 100 in 4=58. dip 74=30+

May 21. about noon.
First 50 in 2=41
Last 50 in 2=28

The 100 in 5= 9. dip 74 = 30

May 23. about $12^h=45'$
First 50 in $2'=40''$
Last 50 in $2=27$

The 100 in $5=7$. dip $74=40$

May 25. about $3=30$
First 50 in $2=41$
Last 50 in $2=30$

The 100 in $5=11$. dip $74=40+$

May 27. about $6=30$ afternoon.
First 50 in $2=41$
Last 50 in $2=28$

The 100 in $5=9$. dip. $74=50$

Uncommon Tumours; *by Mr*. Joseph Atkinson. Phil. Transf. N° 389. p. 340.

ABOUT *Chriſtmaſs* 1723, a maiden about 20 years of age was brought to Mr. *Atkinſon*, who had a tumour on the inſide of her right thigh, that extended from the groin to the knee, and was ſo large, that he judged it contained at leaſt a gallon; the *cutis* was exceedingly diſtended, but of the natural colour, only the capillary veins appeared varicous, and very numerous; ſhe alſo had a large tumour on the buttock of the ſame ſide, as big as a quartern loaf; but when the tumour on the thigh was preſſed, the tumour above was very much increaſed, which ſhewed a communication, and afterwards it proved to be ſo: The patient alſo had another tumour on her right ſide, about the bigneſs of a penny-loaf, that extended from the left ſide of the *vertebræ* of the back to the *hypocondrium*; her body was very much emaciated; ſhe could hardly breathe, and the little victuals ſhe eat paſſed with very great difficulty out of the ſtomach. She had had the *menſes* but twice or thrice, about 12 months before the beginning of thoſe tumours; and it is to be marked, that the tumour of her thigh began firſt, and increaſed almoſt to the ſize he firſt ſaw it at, before the tumour of the buttock and hip began; after that, the tumour of her back began, which, as it increaſed, brought on a great difficulty of breathing. She had been with ſeveral other perſons, who adviſed againſt opening the tumour of her

 thigh,

thigh, most of them being of opinion it proceeded from blood, and that her case was incurable: Mr. *Atkinson* was of a contrary opinion; but declined meddling with it at that time. About two months after he visited the patient again, when the tumours were increased to so prodigious a size, and her body so exceedingly emaciated, that it was surprising she could live under such circumstances: The tumour of the thigh yielded every way to the pressure of the finger, nor was there the least hardness about its extremities; so that it might be easily mistaken for an *aneurysma*, had it not wanted the grand characteristic, *viz.* pulsation, which some say is not to be felt, when the tumours are very large; the middle of this looked a little red, and shining, and seemed to point a little; upon which, he told the people about the patient, that he believed it would break with a small orifice; which accordingly happened three days after, when immediately sending for Mr. *Atkinson*, he found there had been discharged a small quantity of matter, much like what is contained in a *meliceris*; but the orifice was so small or close, that he could not introduce his probe: However, tho' the patient seemed ready to expire, yet at the desire of her parents, he opened this tumour with his lancet, making an incision about an inch and a half long, thro' which poured three quart basons full of matter, besides several smaller ones, which together contained about five quarts; it was very fetid, and bloody towards the latter end of this discharge; upon this, the tumour entirely subsided; insomuch that the thigh instantly became as small as the other: He put his finger into the wound, and found the *fascia lata* quite consumed; the muscles lay all loose; so that he could fairly touch the thigh-bone between them. Immediately upon the discharge of this matter, the tumour on the buttock was considerably abated; but there followed about two or three spoonfuls of florid blood: He dressed it up for this time with a proper digestive and a suitable bandage. The day following he visited the patient again, and found she had slept pretty well, and was much refreshed, and that not the least faintness had attended her during his absence; which shews the imaginary *syncope*, that is apprehended to follow such evacuations, to be groundless. The day following, taking off his dressings, he found the limb but little bigger than the other: At the third dressing there appeared a small hard swelling a little below the orifice, which was occasioned by some grumous blood that lay there, which he turned out with his finger and found to be about four ounces in quantity:

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This was followed by a florid blood, at which Mr. *Atkinson* was much surprised: He then judged this proceeded from some hypogastric vessel that supplied the tumour; and despaired of success, unless he could discover this spring; so in order to find it, he laid open the *sinus* to the groin; and tho' he could not discover the vessel, yet he applied his astringent with such success, that from that time it bled no more: However, there was for a week a considerable discharge of a serous matter, which entirely sunk the tumour of the buttock and hip, and by boulstering and compressing with proper bandages, the so long separated *cutis* closed with the muscles; and all things in about a fortnight seemed to be in a fair way of healing; yet it was near three months before this cure was compleated.

But still (that is, a fortnight after the opening of the thigh) the tumour on the patient's back continued and she was much straitened for breath, alledging, that if it were opened, she should be immediately relieved: Upon which Mr. *Atkinson* opened this tumour (which, as has been said, was as big as a penny wheat loaf) and there issued out upwards of two quarts of matter: He was again very much surprised, that such a quantity (so much more than what possibly this visible tumour could contain) should run out of this orifice; he introduced his probe, and found it penetrate into the cavity of the *thorax*, between the second and third spurious rib, reckoning from below; upon which the patient breathed with all freedom, but there was a *halitus* at this wound: He continued to dress this, and believes, before it tended towards healing, no less (at times) than five quarts of matter was discharged: But when he thought all was over, it filled again and the external tumour became almost as big as before, and the difficulty of breathing as great as ever: So that he now thought all his labour had been in vain: Yet he opened it again with a larger orifice, and from that time dress'd it successfully, till it was perfectly healed. The *menses* returned, the patient continuing in perfect health to this day.

An Experiment to illustrate what has been said on the Figure of the Earth *in* Phil. Transf. N° 386, 387, 388; *by Dr.* Desaguliers. Phil. Transf. N° 389. p. 344.

UPON an axis of iron, that could be made to turn swiftly (by means of a wheel, whose string went round a pulley fixed to the said axis) Dr. *Desaguliers* slipped on two iron hoops, whose planes intersected each other at right angles, representing two colures, which being in a spring-temper, sprung in such a manner

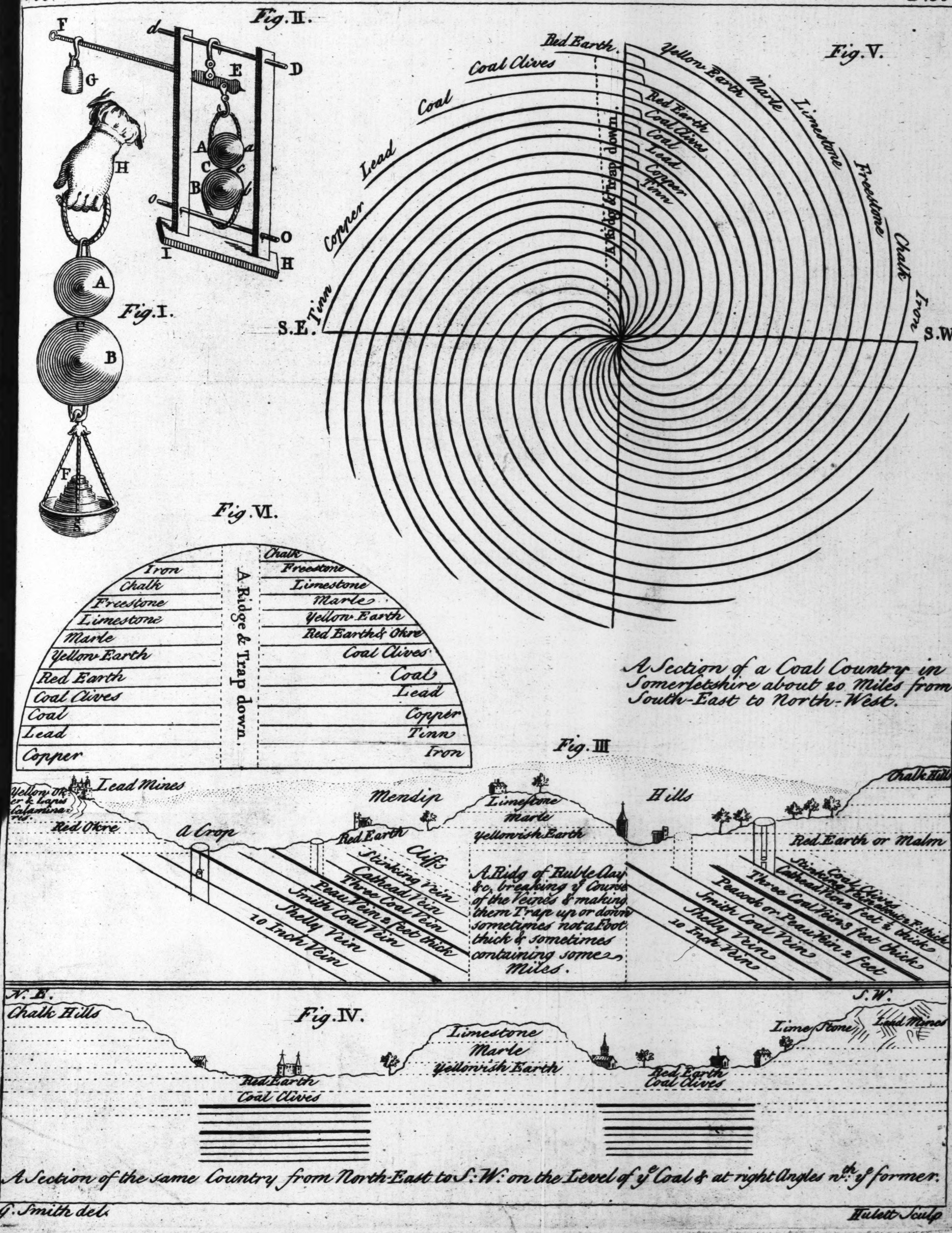
manner as to be $\frac{1}{96}$ part longer in that diameter, that coincided with the axis, than in the equatorial diameter; this proportion being the ſame that M. *Caſſini* ſuppoſes to be between the axis and equatorial diameter of the earth: Two circular plates, to which the ſaid hoops were rivetted, had ſquare holes, thro' which the axis paſſed; ſo that the two poles of the oblong ſpheroid, which the hoops deſcribed in their revolution, might approach together in ſuch a manner, as to let them put on the form of a true ſphere; when, by the whirling, the equatorial diameter of the machine ſwelled, and overpowered the elaſticity of the hoops: A greater degree of ſwiftneſs turned the ſphere into an oblate ſpheroid of Sir *Iſaac Newton*'s figure; a velocity ſtill greater makes the diſproportion of the diameters, ſuch as thoſe of *Jupiter*; and ſtill the equatorial diameter increaſes with the centrifugal force.

Another hoop with a catch, repreſenting the equator, ſhews (in the experiment) the increaſe of the equatorial circumference, and an index, applied to the frame, ſhews the increaſe of the diameter.

Experiments concerning the Coheſion of Lead; *by the Same.* Phil. Tranſ. N° 389. p. 345.

DR. *Deſaguliers* took the leaden balls A and B (repreſented Fig. 1. Plate I.) the firſt weighing 1 pound and the other 2 pounds; and having cut off from each of them a ſegment of about $\frac{1}{4}$ of an inch in diameter, he preſſed them together with his hand, with a little twiſt, in order to bring the flat parts to touch, as much as poſſible. The balls ſtuck ſo faſt, that when the hand H, by means of a ſtring, ſuſtained the upper ball A, the lower one B (by reaſon of its contact at C) was ſuſtained, tho' loaded with the ſcale S, and weights E, which amounted to 16 pounds. Upon adding a little more weight they were ſeparated, and upon viewing the touching ſurfaces, it appeared, that they did not exceed a circle of $\frac{1}{10}$ part of an inch in diameter; but this ſurface can hardly be meaſured accurately, on account of its irregularity. The experiment was ſeveral times repeated, and the coheſion of the balls was different every time.

On the upper pin or bar of the wooden frame D*d*IH (Fig. 2.) the Dr. ſuſpended the ſteelyard EF, whoſe hook held up a leaden ball A of 2 inches in diameter, having a hole thro' it at A to receive a ſtring; the lower ball B, equal to, and prepared in the ſame manner, as the firſt, received the pin O*o* thro' its ſtring; ſo that G, the weight of the ſteelyard, was made



A Section of a Coal Country in Somersetshire about 20 Miles from South-East to North-West.

A Section of the same Country from North-East to S: W: on the Level of ye Coal & at right Angles wth ye former.

G. Smith del. Hulett Sculp.

made uſe of to ſeparate the balls which happened when it was applied at the number 20, in the firſt experiment: But in the three following experiments, the balls were not ſeparated, till the weight was removed to the numbers 25, 37 and 45, expreſſing pounds on the ſteelyard.

Laſtly, the balls being applied together as before (ſtill cleaning the ſurface of contact with his knife, and never making a contact ſenſibly greater than what he mentioned above) the weight G removed quite to the end F, where it weighed 47 pounds, could not ſeparate the balls.

An uncommon Nævus Maternus *or* Mole; *by Dr.* Steigertahl.
Phil. Tranſ. N° 389. p. 347.

Jeremias Rudolph von Walthauſen, a captain of the garriſon at *Danneberg*, near *Lunebourg*, was born *October* 24. 1680, with a very ſingular mole upon his right arm, ſhoulder, and hinder part of his ſide, not unlike the branch of a vine with its leaves and grapes. It was affirmed and atteſted, both by the captain, and ſeveral of his friends and relations, that his mother, when big with child, had an earneſt deſire for grapes, and being impatient to ſtay, till they were full ripe, went down into the garden, to pull off ſome of the unripe; whereupon it happened, that a whole branch, with its leaves and grapes, ſuddenly fell down upon her right arm, at which ſhe was much frighted. Some time after, ſhe was brought to bed, and the child was obſerved to have ſeveral reddiſh or bluiſh ſpots, beginning from behind his ſhoulder, and from thence running over the ſame, down the right arm to the fingers. The captain's whole right ſide was bigger than the left upwards of an inch, and continued ſo to his death. The veins of the right arm were much raiſed, lying almoſt immediately under the *cuticula*, which made them very diſcernible; beſides, they were very much diſtended, chiefly between the elbow and hand, where they were almoſt as big as a man's thumb. On the inſide of the fore-finger, the vein was extended into a ſmall tumour, of a reddiſh, or purple blue, colour, about the bigneſs of a nutmeg, corrugated with ſome lenticular protuberances, which made it in ſome meaſure reſemble a grape. The like tumours, but not ſo big, were obſerved in ſeveral other parts of the arm, in the ſpring time; and as they thought, when the ſap began to enter the vines, as alſo when the vines flowered; and in autumn, when the wine was fermenting he was taken ill with violent and itching pains, in the affected arm, for ſome days. The whole right

right side at that time swell'd more than usual, and the veins and tumours above-mentioned, were so distended with blood, that at last a serous matter was forced out of the pores of the said tumours, which, as it gave the patient some relief; so he promoted it by scraping the tumours with the edge of a penknife. If he held up his affected arm, the running of the blood backwards in the distended veins was very visible; if he held his arm down again, the blood return'd again with some noise, and sensibly fill'd the vessels, which by the preceeding action had been emptied. For this reason, when in bed, he was oblidged to lay his arm upwards. In *February* 1725, he was again taken ill with a violent pain, and a strong sensible pushing of the blood into the affected arm; for both which, they being not only more violent, but continuing longer than usual, he was advised to be let blood on his left arm; as this did not immediately relieve him, they applied, at his desire, externally, *epithemata* of plants, boil'd in wine; this eas'd his pains, and made him somewhat quiet, so that he could walk about the house; but nevertheless he continued restless for some days; and on *Saturday* the 17. of *February* 1725 N. S. early in the morning, he was siez'd with a violent oppression in his breast, which in a few minutes put an end to his life. The surgeon, then present, did not bleed him; but gave him only some spoonfuls of a strong cordial mixture, which he happened to have about him, but he could swallow but little of it, and that with much difficulty. He was 44 years, three months and 24 days old when he died; was otherwise a strong robust man, much used to fatigue.

An Account of the Scythian Lamb; *by Dr.* Breynius. Phil. Trans. N° 390. p. 353. *Translated from the* Latin.

THE vegetable, call'd the *Scythian lamb*, is known among the writers of natural history by the name of *Barametz*, *Borometz*, or *Boranetz*.

The authors who first treated of this vegetable, were *Athanasius Kircher* in his book *de Arte Magneticâ* (who in p. 504, 505, cites *Sigismond L. B. ab Herberstein*, *Hayton Armenus*, *Surius*, and *Julius Cæsar Scaliger*) as also Sir *Francis Bacon*, Lord *Verulam*, *Hist. Nat. cent.* 7. *n.* 609. *Fortunius Licetus de Spontaneo viventium ortu*, *c.* 45. *Andreas Libavius Hist. Agni Scythiæ*. *Eusebius Nierenbergius Hist. Nat. p.* 34. *Adamus Olearius de itinere Persico p. m.* 155. and *Olaus Wormius Mus. p.* 190. not to mention others;

others; among whom are ſeveral botaniſts, who almoſt give us the ſame accounts.

Julius Cæſar Scaliger deſcribes this *Borametz*, under the name of *Agnus Scythicus* (vide *de Subtilitate contra Cardanum Exerc.* 181. §. 29. 30.) to the following purpoſe: ‘ In Zauola the principal hord of the *Tartars*, on account of its ancient nobility, they ſow a ſeed, very much reſembling a melon-ſeed, only not ſo long; from which there grows a plant, called *Borametz*, that is, a *Lamb*: For, it grows in the ſhape of a lamb, almoſt three foot high, reſembling it in the feet, hoofs, ears and the whole head, excepting the horns, inſtead of which it has hairs like horns; it is covered with a very fine ſkin, which the natives ſtrip off and wear on their heads. They ſay that the pulp taſtes like ſea-crabs; and moreover, that when wounded there iſſues forth blood; that it is of an admirable ſweetneſs; that the root ſhoots out of the ground and riſes up as far as the *umbilicus*; that as long as it is ſurrounded with other herbs, it lives; but as ſoon as they are conſumed, it pines and dies away; and that this does not only happen by chance or in proceſs of time, but alſo when they have been cut down for trial ſake; that wolves, and no other beaſts of prey, that feed on fleſh, are eager of it. This I would fain know, how four diſtant legs and feet can proceed from one ſtalk.’

In the ſame manner, other authors deſcribe this plant, or rather tranſcribe the account from *Scaliger*; yet ſome of them vary in ſome circumſtances, and *Athanaſius Kircher* adds, or rather deviſes its figure; nay, the ſkin, as they would have it, was formerly ſhewn in ſome *Muſeums* of natural rarities, as in that of *Wormius*, *Swammerdam*, &c.

Antonius Deuſingius (de Agno vegetabili, quod extat in faſciculo ejus diſſertationum ſelectarum, p. 590. & ſeq.) examining the matter more accurately, ſuſpected the whole a fable; and endeavours to prove, that *Scaliger* himſelf, who was among the firſt that made mention of it, as above, treated it as fabulous; and alſo that others, leſs credulous, have call'd it in queſtion.

And indeed, that this whole account of the lamb, if accurately examined by any unprejudiced perſon, ſavours of the fable, nay really is fabulous, and that *Deuſingius* judged rightly, appears from the following reaſons.

1. Because no such vegetable lamb was ever seen by any credible author. For, what *Olaus Wormius* relates *l. c.* from the account of M. *Eovaldi de Kleiss*, ambassador from the elector of *Brandenbourg* (*viz.* that when he was in the confines of *Tartary*, he was presented by a *Tartar* with a dried plant, with tobacco leaves, to whose stalk hung a fruit, very much resembling the figure of a lamb, a foot long and covered with a curling fleece) is suspicious; since M. *de Kliess* might easily be impos'd upon by the cunning *Tartar*: And what judgment we are to form of the skins, shewn under this name in *Musæum*'s, will appear from what shall be said anon.

2. Because the learned and skilful naturalist Dr. *Engelbert Kæmpfer* (vide his *Obs. de Agno scythico,*) tho' he diligently enquired for this vegetable lamb, in the country where it is suppos'd to grow, yet found nothing like it; his words are to the following purpose, 'because none either of 'the common people in *Tartary*, or of such as are skill'd in 'botany, know any thing of a vegetable animal that eats 'grass, as I have enquir'd with the utmost care, nor is there 'any thing call'd *Borometz*, in that country, excepting real 'sheep; I therefore assert, that whatever is said about this 'plant, is mere fiction and fable.'

3. In fine, because the whole account of this vegetable lamb has so very much the air of fable.

The above-mentioned diligent enquirer into eastern natural history, Dr. *Kæmpfer*, has very well discover'd the origin of this fable, vide *loc. citat.* where (after having premis'd the etymology of the word *Borometz*, which he says is a corruption of the *Muscovite*, *Boranetz*, in *Polish*, *Baranek*, a diminitive of *Baran*, a term of *Sclavonic* origin among the *Poles* and *Russians*, denoting a sheep) he affirms, that in some provinces about the *Caspian* sea, there is a kind of sheep, besides the common sort among us, that differs from it in several respects, particularly the fineness of their furrs, which he describes, and shews how they are prepar'd, for adorning the cloaths of the *Tartars* and *Persians*; and he adds: 'The noble 'and rich, who are ambitious of being cloath'd finer than 'the common sort of people, are fond of the furrs of young 'lambs, which are much more tender than those of the old 'ones, and the younger they are, the dearer; for, their 'wool will bear a fine and close curl, which adds a beauty 'and value to the whole furr. Hence it happens, that they

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'rip up the dam and take out the *fœtus* only for the ſake of 'the furr, which when duly prepared, is of ſo delicate a 'grain, that after cutting off the extremities, it ſcarce reſem-'bles a lamb's ſkin, and might eaſily deceive the ignorant, 'who would be apt to take it for the downy ſkin of a gourd.' *To this he ſubjoins*; 'the price of the furr, according to 'the goodneſs, it is ſuppos'd to be of, is upwards of three 'florins, it ſerves to line their turbants, and frequently by 'way of ornament, it is uſed on the borders of outer garments.' *At length he concludes*; 'whether this fiction was owing to 'the conjecture of ſome philoſopher, or the ignorance of 'the firſt relator, who for want of knowing the language, or 'thro' careleſſneſs, miſapprehending what he heard, or what-'ever elſe might be the occaſion; this animal's furr was 'transformed into a plant:' Thus far *Kæmpfer*.

From what has been cited it appears, that theſe *Perſian* lamb-furrs, called by our furriers Perſianiſche Baranken are of this ſort, tho' not the beſt. *viz.* the furrs of lambs cut out of the dam's belly; for the price of ſuch in *Tartary*, according to *Kæmpfer*, is upwards of three florins, but theſe among us, are ſold for a florin at moſt.

A learned and curious gentleman in his way from *Muſcovy* thro' *Dantzick*, among other natural curioſities, made Dr. *Breynius* a preſent of a *Scythian* lamb, as he call'd it, and which he ſaid was the genuine *Borometz*: It was nearly ſix inches long, with a head, two ears, and four legs, of a brown colour, and downy like velvet, excepting the ears and legs, which were ſmooth, and of a more duſkiſh colour. The Dr. upon examining it, found, that it was neither an animal, nor the fruit of any plant, but the thick, creeping and villous root of ſome vegetable, or rather the creeping ſtalk of ſome plant, that by art was in ſome meaſure formed into an animal; for, the four legs were ſo many pieces of ſtalks, or rather pedicles cut off, which ſupported leaves, as were alſo the ears, which yet were more like horns; beſides, the fibres that ſtuck out here and there, and by which the thick root, or rather the ſtalk, and by this, the plant receiv'd nouriſh-ment, plainly diſcovered the cheat; and upon a more ſtrict examination, he found one of the fore-legs, not continued with the body, as the reſt were, but inſerted by art, as alſo, that the head was very nicely join'd to the neck. So that this lamb was form'd from this root or ſtalk in the manner that puppets are made from the roots of mandrake and

briony: And tho' he was persuaded, it was formed of some plant of the capillary kind, either in *Muscovy* or *Tartary*; yet he could not pretend to determine the species.

Of Camphire; *by Mr.* John Brown. Phil. Transf. N° 390. p. 361. *Translated from the* Latin.

DR. *Neuman* in his account of camphire in the preceeding transaction, asserts among other things; 'I have obtain'd from our own common thyme, a true, dense, crystalliform camphire, that agrees with the oriental in all its qualities, excepting only the smell, and that can stand the test of all the experiments, either hitherto known, or that can possibly be devised &c. *to which a little after he subjoins*, 'that he obtain'd this camphire from the distill'd oil of thyme.' Thus far the Dr. On which Mr. *Brown* makes a few observations.

1. He bethought himself; that we have something among us in *England*, distill'd in the same manner from our own thyme, which hitherto has been improperly call'd the volatile salt of thyme; but he does not see, how it may be more truly call'd the camphire of thyme. Mr. *Brown* does not call in question what the learned Dr. *Neuman* asserted from his own experience about that, whatever it be, which he extracted from thyme, and which he calls camphire; only takes notice of some difference he himself observ'd between camphire, properly so call'd, and that distill'd in *England* from thyme.

His preparation is in the following manner; the coagulated, or condens'd oil of thyme, as pellucid, in the form of crystals, of various sizes and figures; after its first separation it resembles white sugar candy, and immediately after, brown; it smells strong of thyme, is of a pungent or even caustic taste, and not brittle like camphire. Mr. *Brown* owns, that in some experiments this does not differ much from the common camphire, sold in the shops; they both seem to be of the same specific gravity; neither of them can be dissolv'd in water; but both are easily dissolv'd in spirit of wine. But in other respects the phænomena are so different, that in his opinion, the name of camphire cannot agree to both. 1. When camphire is dissolv'd in spirit of wine and afterwards thrown into water, there suddenly floats a-top a white *coagulum*, which really appears to be camphire, in smell, taste and substance: But if you try the same experiment with his coagulated oil, you shall see small drops of oil float at top of the

he water, which are gradually form'd into plane cryſtals (not much unlike thoſe of ſilver) and which likewiſe retain he taſte and ſmell of thyme. The difference appear'd ſtill greater from the following experiment; he expos'd both the above-mentioned mixtures over night in the cold air; in the morning the *coagulum*, which was ſaid to float on the camphire, diſſolv'd in ſpirit of wine, had entirely diſappear'd, whilſt the water ſtill retain'd the ſcent of camphire, probably, on account of the admixture of the ſpirit of wine: But in the other, namely the coagulated oil of thyme, the cryſtals that floated at top the water, remain'd in the ſame ſtate, as before, without any diminution. 2. Who, that is the leaſt converſant in chemiſtry, but knows, that ſolid camphire is changed by means of ſpirit of nitre into a pellucid liquor? But his coagulated oil, if put to the ſame *menſtruum*, quite loſes its priſtine cryſtalline form, and puts on that of a reſinous or gummous ſubſtance; nor will it, tho' you often ſtrongly ſhake the veſſel in which it is contain'd, ever become fluid: Beſides, if you afterwards pour into water, that pellucid liquor, into which the camphire was reduced, the camphire will be renewed: But on the other hand, in his coagulated oil, the ſame reſinous or gummous ſubſtance, will ſtill remain, and the taſte of the thyme continue, with a remarkable bitterneſs added to it. 3. Oil of vitriol (according to Mr. *Boyle*) entirely diſſolves camphire: But it makes no other change in the coagulated oil of thyme, deſcribed above, only that the ſtrongly ſhaking it with the hand, the cryſtals are divided into ſmaller parts; and in this manner, the cryſtals of the oil, if view'd with a microſcope, have a greater reſemblance to little bits of camphire. Moreover, camphire, diſſolv'd in oil of vitriol, recovers itſelf from this *menſtruum*, if it be afterwards pour'd into water; but his ſtill retains the ſame form of the cryſtals, it had in the oil of vitriol, together with the taſte and ſmell of thyme, tho' you pour it into water. 4. Dr. *Neuman* affirms, 'that camphire conſiſts of a rarefied 'inflammable principle'; but that of Mr. *Brown* has none at all: For, if brought near the fire, it immediately melts and becomes a liquid oil. It is true, that if you dip a wick of cotton into it, and bring it near the fire, it preſently catches; but it is evident, that this is owing to the cotton, and is common to all other oils. 5. Fire has different effects upon theſe two bodies: For, any degree of heat will ſublime camphire without melting it, and entirely melt that body

diſtill'd

diſtill'd and condens'd from thyme; and keep it ſo, with little or no ſublimation. Thus far as to the difference: To which Mr. *Brown* ſubjoins ſome teſtimonies concerning coagulated oils or ſalts produced from oils.

In a tranſaction publiſh'd about 30 years before, Dr. *Slare* deſcribes Mr. *Brown*'s oil to the following purpoſe, 'upon 'diſtilling a pretty large quantity of thyme, there is ſepara- 'ted a volatile ſalt of a peculiar nature (which our celebrated 'chemiſt Mr. *Moult* firſt ſhew'd me) pellucid or cryſtalline, 'and as to its ſpecific gravity ſomewhat heavier than water; 'and ſeems to be the ſalt and oil coagulated into one body: 'It does not diſſolve in water, but evaporates ſoon, when 'heated.'

The ſame learned gentleman affirms, 'that he kept oil of 'cinnamon for 30 years; and within the ſix laſt years, it 'precipitated ſome ſalt every year, and that without any 'additament, or uſing any art to give it the form of ſalt.' But as to the nature of this ſalt, whether it be like that which Mr. *Moult* ſhews to be diſtill'd from thyme, the Dr. does not ſay.

Helmont ſpeaks thus of a ſalt, made by art from the ſame oil, 'but when the oil of cinnamon is mix'd with its own 'alkaline ſalt, without any water, it has been entirely changed 'into a volatile ſalt by an artificial, occult circulation for 'three months'. And likewiſe in another place he ſhews the manner in which oil of cedar aſſumes the nature of a ſalt, by means of his *Alkaheſt*, and adds, 'it will incorporate with 'water.'

The celebrated Dr. *Boerhaave* (if the inſtitutions of chemiſtry, lately publiſh'd under his name, be genuine) writes to this purpoſe, 'the moſt precious oils of oriental aromatics, 'when long kept in cloſe veſſels, are changed into a ſalt or 'ſapo; but we cannot eaſily repeat the experiment; for, 'according to *Homberg*, it happens in a courſe of ſeveral 'years, and then they are ſaid to incorporate with water'.

In another place Dr. *Boerhaave* ſeems to agree with Dr. *Neuman*; 'camphire, ſays he, is nothing but an oil con- 'creted by diſtillation into a pellucid maſs: For, it is not the 'production of the camphire-tree alone; but all aromatics 'may yield a peculiar camphire'; and a little after; 'the 'oil, produced in the iſland of *Ceilon*, and elſewhere, from '*cardamom* ſeed, is almoſt entirely changed into camphire, 'which is likewiſe true of ſeveral oils, produced there.'

But

But Mr. *Brown* viewing ſeveral oils diſtill'd in the *Indies*, ſuch as the oil, obtain'd from *cortex caryophyll*, *calamus aromaticus* and the roots of *caſſumun*, &c. could find no condens'd or coagulated matter in them, tho' they had lain by him upwards of ten years; nor could he ſay (as M. *Geoffroy* jun. does in *Mem. de l'Academie Roy.* 1721.) that he ever obſerv'd cryſtals like thoſe of camphire, ſhoot from the oil of turpentine or marjoram, and adhere to the ſides of the veſſels in which they had been kept; tho' he carefully view'd them for ſeveral years.

As to this ſalt or coagulated oil of thyme, other chemiſts in *London* are not unacquainted with it; for, Mr. *Brown* ſaw a ſmall quantity of it (reſembling his) only that it was of a whiter colour: But he never obſerv'd any thing of this kind in any other oils; unleſs, perhaps, he might ſay that it was at that time in its principles, in the oil of mace, which he had diſtill'd 15 months before, in the following manner.

He took a pretty large quantity of mace for one operation, and diſtill'd it for a whole day; but having obtain'd a leſs quantity of oil than he expected, he repeated the operation the ſecond and third day; and the fourth day, after having ſeparated the oil that floated at top the water, he obſerv'd a certain liquor, not mixed with the water, to ſubſide to the bottom of the veſſel; and upon taking it out, he found it was the oil of mace, and heavier than water: So that from the ſame plant, and by the ſame operation, he extracted oil of different gravity; there appeared now ſomething of a cryſtalline form floating on the upper part of this oil, but what ſort of ſubſtance it was, whether camphire or not, time would ſhew.

After the above deſcription of the differences between his coagulated oil of thyme and the oriental camphire, Mr. *John Maud*, a chemiſt, ſhew'd Mr. *Brown* a coagulated oil of marjoram, which he unexpectedly diſcover'd in a veſſel, in which that oil had ſtood five years.

Mr. *Brown* tried the ſame experiments on this coagulated oil of marjoram, as he had done on his coagulated oil of thyme, and found them agree pretty much: So that they equally deſerve the name of camphire.

Effects

Effects of Lightening; *by Mr.* Joseph Wasse. Phil. Transf. N°. 390. p. 366.

WE are told by Mr. *Jessop* in a former *Transaction*, that what the common people call *fairy circles*, are occasioned by lightening: But it has not hitherto been observ'd, that they continue visible for 50 years; and that no composition, us'd in fire-works, will produce near so lasting an effect, as Mr. *Wassee* had experienced. There seems to be something here, which sulphur and nitre will hardly account for. Does it depend upon the large quantity of the matter discharged, or the violence with which it is impell'd? The ground is no ways torn up, and the grass is only a little blasted; which would make one think, its force is well nigh spent: Whereas, when the explosion is near us, the effect is like that of a petard, as appears from the following instance.

At *Mixbury* on *July* 3. 1725, about 2 in the afternoon, one *William Hall*, about 60 years of age, was found dead in a hard gravelly field, together with 5 sheep, which lay round him at about 30 yards distance, which he is supposed to have been driving; of the 5, only that, which lay nearest him, had a visible wound thro' the head: The man lay partly upon his side; the upper part of his scull was terribly fractured, and his right knee out of joint; he had a wound in the sole of his foot, towards the heel; his right ear was cut off and beaten into his scull, and the blood flow'd out of that part upon the ground; all his cloaths and shirt were torn to small pieces, and hung about him; but from the girdle downwards they were entirely carried away, and scattered up and down the field; particularly the soles of a strong new pair of shoes were rent off; his hat was torn to pieces: Mr. *Wasse* had a hand breadth of it, full of irregular slits, and in some few places cut, with a sharp penknife, as it were, and a little singed in the upper part; his beard and the hair of his head were, for the most part, close burnt off; the iron buckle of his belt was thrown 40 yards off, and a knife in the right side pocket of his breeches was broken to pieces, not melted, and the haft split; near each foot appeared two round holes about a yard deep, and five inches diameter, which shews the force of the blow; and Mr. *Wasse* has seen an iron ball shot out of a mortar almost perpendicular, that upon a like gravelly soil, did not make a greater impression. About the time this accident happened, a tradesman of *Mixbury* observed a sort of fire-ball, as large as a man's head,

to burst in four pieces near the church. The storm began at $1^h\ 30'$, and lasted, with intermissions, to $2^h\ 30'$, and we observed the lightening towards *Aylesbury* all the evening. Two persons at *Aynho* were a little hurt at the same time, and one of them struck down to the ground, Mr. *Wasse* himself heard the hiss of a ball of fire, almost as big as the moon, that flew over his garden S. E. to N. W.

Both the abovementioned holes were almost perpendicular for half a yard, and after that became narrower; in both of them the matter was divided into two parts, and formed horizontal cavities about three inches in diameter; in one of them there was found a very hard glazed stone, about 10 inches long, 6 wide, and 4 thick, cracked in two; it could not pierce others, but was turned here and there out of its course; yet it did not leave the least blackness or discolouring any where. It was not the blade, but the haft of the knife, and the hinge that goes into it, that were shiver'd to pieces. Near the sheep which was wounded, the ground was torn up almost two yards round. It was very surprising, that the man's body was not beaten to pieces, or his bones broken at least.

To make a gross estimate of the force, Mr. *Wasse* took a cohorn, charged with $\frac{3}{4}$ of a pound of very good powder, wadded with thick paper, and fir'd it against a stone of the same dimensions, but not so hard, which it shatter'd to pieces at half an inch distance. But in the above mentioned blow, there was above treble the effect, without any discoverable particles at all; and yet it seems to fly like small shot, pierces only here and there, and leaves a good many places quite untouch'd, as was evident from the hat Mr. *Wasse* had by him. To confirm this, one *James Marshal* of *Mixbury* assur'd him, that, in the middle of the same storm, he receiv'd a blow upon his hat, which rattled like shot thro' the branches of a tree; it beat in the crown of the hat a little, without penetrating it; he stagger'd, and was giddy for two days after. Two of his sons were knock'd down to the ground at the same instant, and stunn'd a little; but presently came to themselves, and had no wound. *Query*. Whether this may not be accounted for, by supposing the flame to rarify the air, and make a sort of *vacuum* about one; into which, when it returns again, it gives a stroke, like that of a beetle; as he express'd it: Mr. *Wasse* supposes, that a wind-gun, with compress'd air, wou'd have the same effect, and might easily be tried upon a dog, or some such animal.

Of Magnetical Powers; *by M.* Muſchenbroek. Phil. Tranſ. N° 390. p. 370. *Tranſlated from the* Latin.

IN the firſt place M. *Muſchenbroek* had a mind to try, whether loadſtones operate on each other at different diſtances, according to a certain proportion; and he ſaw in *Phil. Tranſ.* N° 335. that Mr. *Hauksbee* had thought of the ſame thing; but that he had made the experiments with a loadſtone, and needle, that could not ſatisfy accurate enquirers: Whence, notwithſtanding, he concludes in theſe words; 'I 'ſee no reaſon to doubt, but the proportions of this power 'will be regular, and correſpond to the different diſtances.' Which words, as M. *Muſchenbroek* gathers, did not greatly pleaſe all the learned, ſince Dr. *Taylor*, *Phil. Tranſ.* N° 334. repeated the ſame experiments, and made obſervations differing from them.

M. *Muſchenbroek* attempted the ſame thing in a quite different manner; he thus thought with himſelf, that if he took two magnets, and hung one of them by a thread above the other, at different diſtances from each other, and if he tied the end of the thread to a balance, he might weigh the quantity of the force, with which the magnets acted upon each other; which accordingly ſucceeded. He took a very nice balance, and than which, perhaps, there was not a better any where, and put a ſcale to one arm, and a thread ſeveral feet in length to the other; to the lower end of the thread he tied an unarm'd magnet: He made the thread very long, that the experiment might not be diſturb'd by any action of the magnet upon the iron balance; and he therefore, pitch'd upon a place in the houſe, where there was as little iron as poſſible; he took two very good magnets, perfectly ſpherical, (which Mr. *Gilbert* calls *terrellæ*) whoſe poles were exactly in each extremity of the axis of the ſphere; and conſequently, he could very accurately meaſure the diſtances of both poles; he firſt counterpoiſed the magnet, by means of a weight in the ſcale, and afterwards he put both magnets one under the other; and becauſe the balance was moveable, by means of a rope over a pulley, he let it down to different diſtances at pleaſure; and when the ſuperior magnet was attracted downwards by the force of the inferior, he always laid ſo much weight in the ſcale, till the force of the magnet and the weight were in *æquilibrio*: Yet theſe diſtances cannot be meaſured, but by means of a copper body of the ſame length,

as the diſtance between both magnets, on account of the oſcillations of the balance, and becauſe magnets operate leſs at greater diſtances than at ſmaller, and that the *æquilibrium* of the balance cannot be obtain'd, but by ſuch a contrivance. The following table contains the experiments made at the different diſtances of inches and lines; and correſponding thereto are columns, containing the number of grains, which counterpoiſe the attractions at theſe diſtances.

Diſtance inch.	lin.	Grains of attraction.		Lin.	Gr.
13 —	6 —	0		8 —	106.
12 —	0 —	0 $\frac{1}{20}$		7 —	114.
11 —	0 —	0 $\frac{1}{8}$.		6 —	131.
10 —	0 —	0 $\frac{1}{4}$.		5 —	146.
9 —	0 —	0 $\frac{1}{2}$.		4 —	172.
7 —	6 —	1 $\frac{1}{2}$.		3 —	190.
7 —	0 —	2 $\frac{1}{2}$.		2 —	215.
	12 —	70 $\frac{1}{2}$.		1 —	250.
	11 —	78 $\frac{1}{2}$.		$\frac{1}{2}$ —	290.
	10 —	87.	In the very point of contact or	0 —	340.
	9 —	94.			

M. *Muſchenbroek* made uſe of *Rhinland* inches, and the grains were apothecary's weight, which he firſt very accurately examined, in order to have them true, and of equal weight.

He made theſe experiments on *December* 24. 1724. and that with the utmoſt attention: So that he could hardly expect to have them made with greater accuracy.

But whether we can in any meaſure gather from theſe, that there is a proportion between the diſtances and forces; he does not ſee.

After he had proceeded ſo far, he ſuſpected, whether, the ſuſpended magnet were not in ſome meaſure heterogeneous, and whether another, ſubſtituted in its ſtead, might not render the event more ſucceſsful, and from which he might at leaſt receive more light; for, theſe experiments were too tedious, to have reaped but ſo little advantage from them. The following table exhibits the obſervations he made with another very good ſmall magnet, whilſt the inferior *terrella* was the ſame that was made uſe of in the preceeding experiments, and firmly fix'd on a table: Theſe experiments were made in the ſame manner as the former.

 Diſtance

Distance inch.	lin.	Grains of attraction.
5 —	10 —	$1\frac{1}{4}$.
4 —	6 —	$2\frac{1}{4}$.
3 —	9 —	3.
2 —	4 —	9.
1 —	9 —	12.
1 —	0 —	23.
	11	$23\frac{1}{2}$.
	10 —	$26\frac{1}{4}$.
	9 —	29.
	8 —	$30\frac{3}{4}$.

Lin.	Grains.
7 —	33.
6 —	$38\frac{1}{2}$.
5 —	$43\frac{1}{2}$.
4 —	$50\frac{1}{2}$.
3 —	62.
2 —	79.
1 —	140.
$\frac{1}{2}$ —	186.
0 —	340.

But here again occur very great irregularities, from which nothing can be concluded: This only is ſurpriſing, that tho' the magnet, made uſe of in the ſecond experiment was ſmaller than that in the firſt; yet in the point of mutual contact it was attracted with equal forces, namely 340 grains; whilſt in other diſtances the attraction was much leſs, as appears from comparing both tables; but beſides, this ſmaller magnet, made uſe of in the ſecond experiment, was more vigorous and much better at raiſing the iron, than the magnet in the firſt.

He repeated theſe experiments with other magnets, and particularly with one, whoſe force was ſo great, as to affect a magnetic needle, at the diſtance of 14 *Rhinland* feet; and he did not know of a magnet like this, deſcrib'd any where: But from all the experiments he could only conclude, that there is no proportion between the forces and diſtances.

Since both the declination and inclination of the magnetic needle vary almoſt every year, he had a mind to obſerve, whether the force of the magnet was the ſame every day, or greater or leſs in ſummer than in winter; but he found by ſeveral experiments, that the force is leſs in ſummer than in winter, at leaſt in the ſummer of 1725; whether it will be the ſame in 1726, muſt be then diſcovered.

He therefore took the two magnets made uſe of in the firſt experiment, and on the 25. of *July* 1725, when the barometer was at $29\frac{5}{24}$ inches; he made experiments with them altogether in the ſame manner, and in the ſame part of his houſe as before. M. *Fahrenheit*'s thermometer was at 62 degrees, the wind N. W. the ſky ſerene, and the weather dry.

Diſtance

Distance inch.	lin.	Grains of attraction.	Lin.	Gr.
12	0	0.	7	106.
9	0	$1\frac{1}{8}$.	6	111.
8	0	$1\frac{1}{2}$.	5	132.
7	6	2.	4	149.
7	0	$2\frac{1}{2}$.	3	173.
	12	$70\frac{1}{2}$.	2	205.
	11	$75\frac{1}{2}$.	1	240.
	10	85.	$\frac{1}{2}$	270.
	9	92.	0	300.
	8	100.		

Philosophers, it is true, are agreed, that both poles of the magnet do not act with equal force; but that the north poles are stronger than the south; this has been asserted indeed, but no where accurately demonstrated; and because M. *Muschenbroeck*'s method of estimating the magnetic forces, was sufficiently easy, and that by means thereof, this might be accurately determined; he turn'd both the poles of each magnet in such a manner, that the corresponding poles might be opposite to each other; the observations he made on the magnets in this last experiment, are as follows.

Distance lin.	Grains of attraction	Lin.	Gr.
12	57.	5	101.
11	63.	4	113.
10	66.	3	124.
9	70.	2	148.
8	79.	1	168.
7	83.	0	228.
6	90.		

Hence it evidently appears, that both poles of the magnet do not act with the same force; the quantity of the difference may be seen by comparing both these tables together.

Since M. *Muschenbroek* had hitherto been persuaded, that the action of the magnet depends on effluvia, or at least on some impelling fluid without the magnet; and since he had observed, that the most learned philosophers were of the same opinion, he had a mind to try, whether he could confirm this by any experi-

experiment. While he, therefore, made the former experiments with magnets, placed at different diſtances from each other, he interpoſed very thick pieces of lead, tin, ſilver, copper and a pretty large maſs of mercury, in order to ſee, whether the magnetic effluvia would not be intercepted; and if not entirely, yet if in ſome meaſure at leaſt: Glaſs is pellucid and tranſmits the rays of light, but not in ſuch quantity, if no glaſs intervened; in the ſame manner he ſuppoſed, that the magnetic effluvia, if they were not quite intercepted, yet in ſome degree would hinder the magnets from attracting with ſuch force, if a piece of lead of a cubical foot, or a piece of lead two inches thick, the tin of the ſame thickneſs, and afterwards copper or a large maſs of mercury, intervened between them; but he obſerved, that whatever bodies he interpoſed, the magnetic forces were always the ſame, as if no ſuch bodies at all intervened; which he thinks indeed a thing ſurpriſing and not underſtood by any mortal: For, we are not to ſuppoſe, that theſe bodies are ſo porous as to have no ſolidity; if therefore, they have ſome ſolid parts, as they have a great deal, ſhall not theſe hinder the approach of a foreign fluid, or its egreſs from the magnet (he does not ſay that they will intercept all the fluid) but ſome of it at leaſt; but experiments ſhew, that the magnetic forces are noways hindred) or ſhall theſe effluvia be much more ſubtile than the rays of light? Beſides, that this again is but an hypotheſis, the above difficulty is not removed: Fire is intercepted by bodies, and light does not immediately penetrate all bodies; and thus it is with all fluids, they meet with reſiſtance from ſolids; but it is not ſo with the magnetic effluvia, they meet with none from a ſolid body; and this is the grand difficulty.

But he takes the ſtrongeſt argument from the repelling forces of magnets, which are much weaker than the attracting forces, as ſhall appear anon from experiments; ſo that a fluid muſt neceſſarily come from without towards the magnet, which meeting the other magnet, impells the one fluid towards the other, and which enters the magnet; and becauſe the magnetic attraction is much ſtronger than the repulſion, a greater quantity of the fluid enters into the magnet than paſſes out from it: Whence the magnet muſt neceſſarily be ſoon filled with this fluid, ſo as to be no longer porous; nor can it be ſuppoſed, that this fluid is emitted from all parts of the magnet, as it were; for, the attraction is in every point of the magnet, but the repulſion is only in the poles. In order to ſhew that the magnetic

magnetic repulsion is less than the attraction, the following table contains the experiments, made with the last mentioned magnets.

Distance inch.	lin.	Grains of repulsion		Distance inch.	lin.	Grains of repulsion
13 —	0 —	0.		1 —	11 —	16.
11 —	11 —	$\frac{1}{2}$.		1 —	10 —	17.
10 —	9 —	$\frac{1}{4}$.		1 —	4 —	17.
9 —	9 —	1.		1 —	0 —	24.
9 —	0 —	1.			10 —	24.
8 —	0 —	1 $\frac{1}{4}$.			7 —	25.
7 —	0 —	1 $\frac{3}{4}$.			6 —	25 $\frac{1}{2}$.
6 —	1 —	2.			5 —	27 $\frac{1}{2}$.
5 —	1 —	3 $\frac{1}{2}$.			4 —	29.
4 —	0 —	6 $\frac{1}{2}$.			1 —	34.
2 —	9	11 $\frac{1}{2}$.	in the very point of contact		0 —	44.
2 —	3	13.				

Hence may be seen that no proportion can be deduced from these experiments on the repulsion of magnets; but that magnets are, indeed, very surprising bodies, of which we hitherto know but very little.

An Account of the anomalous epidemic Small-pox; *beginning at* Plymouth *in* August, 1724, *and continuing to* June 1725; *by Dr.* Huxham. Phil. Transf. N° 390. p. 379.

THE small-pox were preceeded by the usual symptoms of that distemper; but the pains of the limbs and back were generally more severe than usual, as were likewise the *nausea* and vomiting. A great many were seised with violent colic pains, which would leave them upon the eruption, or after a glyster or two, with a gentle anodyne: The stools were commonly bilious. It sometimes happened, that the symptoms would not seem very severe before, and at the eruption; and yet the pox would prove very confluent and fatal at the state of the distemper: The pustules were very small, and did not fill regularly; but in a day or two after the eruption, they would flatten and be depressed in the middle; the Dr. observed this, even in the distinct kind. In some patients they appeared in less than 24 hours from the infection: When they broke out so very soon, they were always of the confluent sort, as is commonly observed. The eruption was attended with prodigious sneezing

neezing, especially in children. The Dr. saw a child about five years of age, that sneezed incessantly for more than 30 hours, nor could it be allayed but by anodynes. This child had the confluent sort, and died the 13th day. In some patients both at and after the appearance of the pustules, they would itch most intolerably; which also happened to the child just mentioned, and was generally a bad symptom, as it was an argument of the great acrimony of the morbific matter. In some few patients, a day or two after the eruption seemed to be compleated, there would appear in the interstices of the small-pox several miliary glands, some of a dark red, others filled with a limpid *serum*: These never came to suppuration, as the secondary crop of small-pox, which the Dr. now and then observed sometimes do; nor were they as large. Tho' this be a bad symptom in general; yet in a girl seven years of age he perceived her fever and *delirium* go entirely off upon this eruption, and the urine immediately settled. Some patients had a great many purple *petechiæ* appearing among the small-pox at the eruption, and the pustules would look of a livid hue: In others the purples would not discover themselves, till the maturation. The Dr. knew but one patient, that had these spots, during this constitution, that survived the distemper; but some died the 5th or 6th day; some dwindled on till the 10th or 11th. During the suppuration, the pox would become very sessile, and the coherent kind would enlarge their bases exceedingly: So that tho' they seemed for some time after the eruption to be very distinct they would now flux together. A purple speck would often appear in the center of the pustules, which would spread and grow blacker and blacker by degrees. The interstices would also sometimes turn pale, sometimes livid; which are very bad symptoms. The pustules that had not the purple speck, did not incrustate yellow, but appeared of a dead ash-colour, and by degrees grew into a dark black crust.

The salivation which should constantly accompany the maturation in the confluent sort, was in several patients very inconsiderable, in some none at all, excepting only a very small quantity of exceeding viscid matter, that was got off by syringing. The Dr. had under his care two adult persons and some children, labouring under the confluent sort, who neither salivated nor purged; except when some lenient cathartics were given them; and yet they got over the distemper. Indeed, it was very rare to find children have that gentle *diarrhœa*, which *Sydenham*, and others, justly reckon, supplies the salivation in persons

persons of more advanced age. Some very young children, on the contrary, drivelled exceedingly thro' the course of the distemper. In two children, one of five, the other of seven years of age, no salivation came on, till after the 13th day; and then it was in such quantity, and continued so long, that it was with difficulty he put a stop to it, first by purges, and then by the bark, astringents, *&c.* To the younger of these, it is true, he gave four grains of calomel; but it was soon purged off. Where the swelling of the face and throat was very hard, painful and tense, with a strong vibration of the carotid arteries, and little or no salivation, the patients generally grew delirious at the state of the distemper: These symptoms frequently proved fatal. The maxillary and parotid glands of such as recovered, would remain swoln, and indurated for a considerable time, after the entire desquammation of the pox (tho' that were very slow) nor would these tumours go off, but after repeated purging, and that with calomel, *&c.* Those tumours were undoubtedly the consequence of a very viscid matter, obstructing those glands; which hardened the swelling of the face, hindered the salivation, and in some measure, the circulation thro' the external carotids; by which means more blood being forced thro' the external carotids, an inflammation of the brain, and a *delirium* might be partly brought on; and this likewise happening, when the blood was fraught with acrimonious matter, absorbed from the pustules, rendered the *delirium*, at that state of the distemper vastly more dangerous than in the *apparatus*, when it happened almost of course. Under these circumstances, bleeding, emollient glysters, eccoprotics, and plentiful dilution were absolutely necessary. On this occasion it may be asked, whether or no the salivation being very viscid and defective, the tumour of the face hard and tense, some mercurial (as a duly prepared calomel) might not be given with advantage, even in the state of maturation? The Dr. has frequently given cinnabar to good purpose: There are some instances that would seem to justify such a practice; and he knows but one material objection to it, and that is, that the weight of the mercury would, by increasing the *momentum* of the motion of the blood, augment the fever; but surely we have given calomel after the incrustation, when, the secondary fever hath subsisted, without any manner of ill consequence, nay, with great success. Nothing so certainly fuses viscous tough humours, as this, when joined with plentiful diluting liquors; and so prepares them to be discharged by the proper outlets:

As to *oxymel ſcillit.* ſyringing, &c. in a defective ſalivation; the former, it is true, by puking, ſometimes irritates the glands of the *membrana Schneideriana* to diſcharge their contents; ſyringing barely deterges the orifices of the ſalivary ducts: Neither have but little certain effect farther; whereas the viſcous obſtructing matter is lodged in the innermoſt glands, and even in the blood itſelf. This method ſeems peculiarly adapted to ſuch an epidemic ſmall-pox, the Dr. is now deſcribing, in which there were all the indications imaginable of a very viſcid ſtate of humours. The blood, when drawn, was always exceeding viſcous, eſpecially at the ſtate of the diſeaſe: Frequently there was little or no ſalivation; generally it was extremely glutinous, ſo that the nurſes were ſeveral times obliged to pull the matter out of the patients mouth with their fingers; and without drinking very plentifully, it would ſoon ceaſe. A *diarrhæa* very ſeldom happened to children. The bliſters ſoon dried up. The Dr. heard of no one during this conſtitution, that made bloody urine. Where that dreadful ſymptom happened, the *craſis* of the blood ſeems to be diſſolved (as *Lyſter* well obſerves) on the contrary, the recited ſymptoms argued a too compact and viſcous *diatheſis* of the blood. The ſtate of the humours, during this conſtitution, might in part, at leaſt, depend on the extraordinary drineſs of the ſeaſon, and the almoſt conſtant northerly and eaſterly winds, which we had in *October*, *November*, *February* and *March* 1725: From the middle of *January* to the middle of *April* was a drier ſeaſon than ever was known in this country, where we certainly have, in general, more conſtant rain than in moſt places in *England*; *Plymouth* being noted for wet weather.

This remarkable change of the temperament of the air muſt undoubtedly have ſome conſiderable effect on human bodies; a very cold wind ſuffering only the thinner part of the blood to paſs off by perſpiration; nor in ſuch ſeaſons, does the body imbibe (as *Keill* obſerves) ſo much of a diluting humidity from the air: Hence the neceſſity of drinking plentifully thin diluting liquors, which, as it is always proper in this diſtemper, ſo when it happens in ſuch a ſeaſon, is highly neceſſary: And the Dr. is of opinion, M. *Andry*'s method of bathing in warm water and milk, or warm milk, before the eruption, may, upon ſeveral accounts, be proper in ſuch a temperament of the air. There can be no objection againſt it, but its not being in faſhion. The Dr. took particular notice, that while, and juſt after the eaſterly winds blew exceſſively ſtrong for ſeven or eight days

together

ogether in *October* and *November*, the patients he then ſaw in he ſmall-pox, ſcarce ſalivated at all; particularly an adult perſon, who had the confluent pox very ſeverely, did not ſpit the eaſt thro' the whole courſe of the diſtemper; ſhe was ſeiſed with a violent pleuriſy the 18th day, but was relieved by bleedng: The blood was the moſt viſcid the Dr. ever ſaw. It is emarked by *Lanciſi*, that people expectorate very little in diſorders of the breaſt, when cold, dry, eaſterly winds blow; and his is what the Dr. frequently obſerved: And this may be one eaſon, why ſome aſthmatic people generally ſuffer a paroxyſm t ſuch ſeaſons. The ſwelling of the hands did not ſo reguarly ſucceed the detumeſcence of the face, during this conſtituion, as he obſerved in other kinds of epidemic ſmall-pox. Some had very ſmall, or rather no tumours at all. It was very rare hat the legs and feet ſwell'd, till after the patients ſat up, and hen they had much pain in the parts. The Dr. often reflected n it, whether the ſucceſſion of the tumours in the hands to that of the face might not partly depend on the later inflammation nd ſuppuration of the puſtules of thoſe parts: The pain and inflammation being a *ſtimulus* determining the humours to the affected part: And it is particularly to be obſerved, that the greateſt pain of the hands and arms commonly happens at the ime, when the ſalivation begins to ceaſe: So that the tumour of the hands may, in ſome meaſure, prove a *ſuccedaneum* to he ſalivation. It is the common obſervation, that the puſtules of the arms and hands inflame and maturate a day or two later han thoſe of the face; and thoſe of the legs and feet lateſt, which may alſo be the reaſon, that the tumour of the legs ſucceeds that of the hands. The Dr. has been the rather inclined o this opinion; inaſmuch as he has ſometimes obſerved a conſiderable ſwelling of the hands (the puſtules being very painful and inflamed) and that too in the diſtinct kind, when there hath been little or none in the face: Generally the more painful a boil is, the greater the tumour around it; and conſequently, he tumour of a part is in proportion to the painfullneſs of the boils, and their number. Whence the Dr. would inforce the uſe of epiſpaſtics, applied above the wriſts, a little before the ime we expect the tumour of the hands ſhould ariſe (eſpecially when ſymptoms are threatning) as they are *ſtimuli* to be depended on, not only attenuating and deriving the humours to he parts, but alſo diſcharging them; and ſo proving a proper outlet to the morbific matter, which before was thrown off by he (now partly ſuppreſſed) ſalivation. Bliſters applied to the

neck frequently relieve the extreme pain of the throat, and difficulty of ſwallowing, which ſometimes are exceeding troubleſome to the patient in the third ſtage of the ſmall-pox, by drawing the humours another way; nay, in ſome, where veſicatories have been early applied, and the parts continued to run exceſſively, there hath been leſs ſwelling and leſs ſalivation, than ſeemed proportionable to the vehemence of the diſtemper, but without any diſadvantage to the patients; the running of the bliſter ſupplying the defect of the ſpitting. It ſeems then but reaſonable, when we expect the tranſlation of the noxious humour to the hands, which is what nature itſelf affects, to endeavour to promote its flux thither, and give it vent. How advantageous diſcharges of this nature may be, the Dr. had occaſion ſome years before of obſerving in the caſe of a lady, where thro' the prodigious diſcharge of bliſters, applied to the patient's neck, ears and arms; as alſo a plentiful flux of urine, ſhe neither ſwelled, nor ſalivated, thro' the whole courſe of a very dangerous confluent ſmall-pox, and yet recovered. Any perſon, that hath been converſant in practice, cannot but have obſerved tranſlations of the morbid matter from one part to another, ſometimes of the greateſt ſervice, eſpecially, where it hath had a diſcharge (indeed, all critical evacuations are of this nature) but the Dr. means, how often hath a boil, an impoſthume, or ſwelling of the limbs been the evident means of terminating a fever? This he particularly experienced in himſelf, ſeveral years before at *Paris*, when labouring under a violent, inflammatory fever with a *delirium*, the ninth day towards night, he was ſeiſed with exceſſive pain in his arms and hands; upon which, by perſuaſion of two of his fellow-ſtudents, he bathed his hands a long time in warm water. In a little while his hands began to ſwell, and in four or five hours his *delirium* and fever went off entirely, tho' his hands continued ſwoln and pained for ſome time. If nature, therefore, in ſome caſes, take ſuch extraordinary methods to free herſelf from diſeaſes, how intent ought we to be in promoting her operations, in a diſtemper, where the *metaſtaſis* of the morbific matter to the hands and feet is generally regular and ſalutary. It is, undoubtedly, upon this view, that *Baglivi* orders ſponges, ſoaked in a warm emollient decoction, to be applied to the hands and feet in the ſmall-pox; and this, he affirms, he has done with good ſucceſs. The Dr. has obſerved no leſs from bliſters, maturely applied to the arms and legs; but then he ordered the patients to drink plentifully of a thin whey, or the like, which takes off, in

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great meaſure, the acrimony of the *cantharides*. Here the Dr. obſerves, that the *delirium*, attending the eruption of the ſmall-pox, is very much alleviated by the application of emollient cataplaſms to the feet, eſpecially in children; and this is what the Dr. himſelf commonly of courſe applied when conſulted at the beginning of the diſtemper; and he thinks he has had reaſon to imagine, in ſeveral caſes, that it hath been a means of deriving the variolous matter that way; and, by making the eruptions more copious in the lower parts, the face and breaſt ſuffered leſs, than he had reaſon to apprehend, otherwiſe might have happened. The great tenderneſs of the feet, which happens after their application, is a trifling diſadvantage, in compariſon of the benefit may be received by them; and ſo are thoſe ſhooting pains, which often affect the legs, on the uſe of thoſe cataplaſms; not to ſay, that theſe are rather an argument of the benefit, ariſing from their uſe.

In the confluent ſmall-pox, generally a micturition and dyſury came on, about the 12th or 13th day; and that when no bliſters were applied. If a large quantity of turbid urine follow'd, it was ſoon ſucceeded by urine, which depoſited a great deal of ſediment; but if it proved thin and limpid, and in ſmall quantity, a *delirium*, *tremor*, *ſubſultus tendinum* and other convulſive ſymptoms, ſoon enſued. No ſymptoms were ſo certainly fatal at the turn of theſe ſmall-pox, as a *delirium*; and (what is, he thinks, a conſtant bad omen in all kind of eruptive fevers at the ſtate) a *dyſpnœa*, or *anheloſa reſpiratio:* Bleeding, upon the firſt appearance of them frequently ſaved the patient; and the omiſſion of it for a few hours made the caſe irrecoverable. It was very common in perſons afflicted with this ſort of ſmall-pox, for 8 or 10 puſtules to run together, and form a large veſication, full of a limpid, crude matter, which would continue ſo for ſeveral days after the incruſtation. In one that died, he obſerved mortifications under theſe bladders. He thought it neceſſary to let out this matter with a lancet or needle, as ſoon as poſſible; leſt it ſhould (as it did, when left to itſelf) cauſe an ulceration; and he could not but think it proper, in regard the thinneſs of the matter, rendered it capable of being abſorbed into the maſs of blood; and that the longer it lay, the more acrimonious it would grow, as not admitting of concoction. In two patients he obſerved ſeveral of the puſtules filled with a bloody *ſanies:* He was ſurpriſed to find one of them get ſo eaſily over the diſtemper, tho' ſhe alſo laboured under the flux-kind. The deſquammation was very ſlow, the black

black crusts adhering for several days, nay, weeks, after the turn, while a great deal of purulent matter gleeted from under them: These left very ugly *cicatrices*. In this case, no application seemed to the Dr. to have a better effect, than frequently fomenting the parts with warm milk, or milk and water; this diluted the acrid salts, washed them off, and softened the skin: Oily liniments, by stopping the pores, are frequently hurtful. In a case or two he observed a repullulation of pustules under the crusts in the face and hands, when thrown off; particularly in the abovementioned boy, that recovered with purple spots. The latter were distinct, tho' the former were for the most part confluent. Nothing so certainly abated, and took off the secondary fever after bleeding (if indicated) as gentle cathartics, such as rhubarb, manna, tartar, *infus. senn.* &c. The hot, scammoniate, aloetic purgers seem not so proper, at least, to begin with. These he gave the 10th, 11th, 12th or 13th days, if he found the patient have a quick pulse, feverish heat, dry tongue, head-ach, restless anxiety, and other symptoms of the putrid fever. Some one or other of these being once or twice repeated, he gave calomel, and purged it off. This was the general method, and the most successful. The Dr. cannot but think the world highly obliged to Dr. *Friend* and others of the faculty, that have introduced and writ in favour of this method. In the beginning of his practice, the Dr. relying upon the authority of *Morton*, gave the *bark* to check the secondary fever; especially, when he found it (as is very frequent) evidently intermit; but he could not say with a success anyways answering his expectation: Not but after due purging, the bark is very proper to extinguish the hectical disposition of the blood, which is frequently the consequences of the small-pox; to which if a cool regimen, and asses milk (where no idiosyncrasy forbids it) be subjoined, we have done, perhaps, as much as lies in the power of physic. By this method of early purging in the coherent and confluent small-pox, the Dr. has observed several rescued from the most imminent danger. This, certainly is the only way of cleansing the first passages, stuffed with a load of fetid, acrid, impurities, thrown off by the glands of the guts, which cannot be supposed to cease from performing their office, during the course of this distemper: And in regard the pores of the skin are at this time very much constipated by the incrustated pustules, it is reasonable to think, that the glands of the guts rather separate more than usual; it being an allowed maxim in physic, that the lessening one evacuation is the increase of another;

nother; eſpecially, where there is ſuch a peculiar conſent, as etween the ſkin and the guts. If ſo, the excrement, being re- ained for a week or more, and by its weight preſſing on the reat artery, hinders the blood from paſſing freely to the lower arts, and ſo overcharges the brain: Hence thoſe *delirium's*, *oma's*, &c. ſo frequently threatening at this ſtage of the diſtem- er: Farther, can we imagine, that the putrid recrement of the ow putrid blood joined, perhaps, with the *pus* of the internal ſmall-pox, and alſo with the addition of ſome part of the orbific matter, ſeparated by the glands of the *fauces*, which s accidentally ſwallowed, muſt not be greatly prejudicial, by emaining in the inteſtines? Where growing more and more crimonious (as is the nature even of our moſt balſamic juices, when out of the reach of the circulation, and expoſed to the onſtant heat of the body) it contaminates the chyle, or liquors, hat are drunk, is re-abſorbed into the maſs of blood, and be- comes a *pabulum* to the very fever, which nature endeavours, even this way, partly at leaſt, to throw off. And, indeed, what offenſive, fetid, large ſtools do we obſerve in this diſtem- er, on the uſe of glyſters, and more eſpecially after a purga- ive, at the ſtate, or declination? So that this matter, lying ong in the guts, and growing more and more putrid, becomes t laſt ſo virulent, as to corrode them, and bring on that very *diarrhœa*, or dyſentery, which, it is ſo vainly apprehended, would ariſe from a gentle cathartic; ſince we can always check its force (if needful) with an opiate, &c. How often do we find feveriſh, hectic heats proceed from a cacochymy of the firſt paſſages, eſpecially, in children; in which caſe, a little rhu- barb, or a few gentle ſtomachic purgers, ſhall do more to re- move the feveriſh diſpoſition than a pound of the *bark*. It is true, it is common enough to find the *bark* itſelf purge gently upon its firſt adminiſtration; and the Dr. is confident, in ſome caſes, it hath the better effect. Have we not obſerved ſome in- termittents cured by one ſeaſonable vomit? And that not barely by the ſhock, and agitation, given to the blood-veſſels, *genus nervoſum*, &c. and ſo acting as an attenuant; but by throwing off the *ſaburra* from the ſtomach, which fed the feveriſh pa- roxyſms; and this is more particularly evident in the fever, fre- quently ſupervening a ſurfeit. Hence it is, that a ſmall quan- tity of the bark ſometimes does more, after a vomit, than a much greater could, before it. Why, therefore, when there is a lodgment of putrid matter in the firſt paſſages, which, in part, at leaſt feeds the ſecondary fever, ſhould not we attempt

to

to carry it off, either by gentle vomiting, or purging, as may be judged most convenient? It hath, and may be objected to this practice, that it tends to draw the noxious humours from the circumference to the center: But to this it hath been answered, that the purging is more especially pleaded for, when the incrustation is begun, and the matter too thick to be absorbed. If nature, neither by her own effort, nor the help of art, is capable of keeping the morbific matter from falling on the more vital parts, but by an unfortunate translation of it, is like to sink under its weight; as upon a sudden retrocession of the tumour of the face and hands, a premature suppression of the salivation, &c. doth it not seem necessary to endeavour to carry off the offending matter by some other outlet? As in the present case, by the guts, which are much more easily sollicited to a discharge, than either the pores of the skin, the urinary passages, or salivary ducts. Indeed, when the salivation of course ceases, it seems necessary (in the Dr's opinion) to promote some other evacuation in its room. At the same time, *cardiac*, or *alexipharmic* medicines are (if judged needful) in noways contra-indicated by this method: Nature affects this way in children, to whom a gentle *diarrhœa* is commonly of the greatest service, as proving a happy substitute to the salivation in older persons. The following case will evince, how necessary it may be sometimes to evacuate an offending acrimonious matter, lodged in the guts, and that too, even in the midst of the suppuration. The Dr. had under his care a patient of about 30, ill of the small-pox. It happened, that the 4th day from the eruption, he was seised with a violent bilious colic, to which he had been formerly subject; this threw him into the utmost agony; his pox flatted and grew pale, as also the interstices; his pulse was exceeding languid, and he had a prodigious tremor, with clammy sweats. The Dr. ordered two glysters to be thrown up, the one as soon as the other was rendered; these gave him five large bilious stools: After the third stool, he was tolerably easy: However, he ordered him *Laudan solid. gr.* 1*ss. Croc. Anglic.* gr. iv. *Theriac. Andromach.* ℨ*ss.* every 4th or 6th hour, to be washed down with a testaceous julep. The patient took the *Laudan.* three times, and slept sound all night. The next morning the pustules were round, florid and turgid; the patient got over the distemper, tho' he relapsed into his colic, some days after the turn, which, upon purging with calomel, &c. and the use of opiates, soon left him. This patient, both before, and at the eruption, complained of a great

difficulty

difficulty of breathing, with a ſhort importunate cough, and a violent pain under his *ſternum*; for which reaſon the Dr. order'd 16 ounces of blood to be drawn, which was very fizy.

The greater part of the adult perſons, that had been ſeiz'd with this diſtemper, died; among whom was a gentlewoman of 72 years of age; a very uncommon exit for a perſon of her years!

It was a remarkable inſtance of the extraordinary virulence of this ſort of ſmall-pox, that the women (tho' they had had the ſmall-pox before; and ſome of them very ſeverely too) who conſtantly attended thoſe ill of the confluent kind, whether children, or adult perſons, had generally ſeveral puſtules broken out on the face, hands and breaſt, exactly reſembling the pocky puſtules; which, undoubtedly, aroſe from the matter of the cruſh'd pox, infecting the ſkin in thoſe parts. Thoſe puſtules aroſe, maturated, and ſcabb'd off, entirely like the true ſmall-pox. The Dr. knew one woman, that had more than 40 on one ſide of her face and breaſt, the child ſhe attended, frequently leaning upon thoſe parts on that ſide. He obſerved, that thoſe, who had the tendereſt ſkins, and who attended thoſe ill of the worſt ſort, had moſt of thoſe eruptions: There were a great many inſtances of this nature.

In the beginning of this conſtitution, the ſmall-pox were much more malignant than they had been for a month or two before. It is true, when they raged moſt ſeverely, ſome children had them very favourably, and required no other phyſick than to be duly purged after them.

An Account of the Strata *in Coal Mines,* &c. *by Mr.* Strachey. Phil. Tranſ. N° 391, p. 395.

MR. *Strachey* here corrects a typographical error in his account (in *Phil. Tranſ.* N° 360) of the ſeveral *ſtrata* of earths and minerals, found in ſome of the coal works in *Somerſetſhire*; for, whereas he ſaid, *that in thoſe parts they never meet with freeſtone over the coal*, it is by miſtake, call'd fire-ſtone; whereas the latter is always found in thoſe mines, contrary to what happens in the works in *Staffordſhire*, *Newcaſtle* and *Scotland*, where freeſtone does, it is true, lie over the coal. Mr. *Strachey* farther obſerv'd the *ſtrata* of ſtone, clay and marle of the interjacent hills; where, under the black marle, there lies a ſpongy, yellowiſh earth; all

this lies over the red ſoil, which, as he has ſaid, is ge-
rally the ſurface of the vallies where the coal is found; an
this red mould on the ſurface, degenerates into marle
loom, ſo towards the north-eaſt, beyond, or without t
veins of coals, about *Winford* in the ſame county, it turns
ruddle, or red-ocre, chiefly us'd for marking of ſheep, a
for ground colours, or priming, inſtead of *Spaniſh* brow
and often counterſeits bole armoniac.

But as he never heard any coal was found to the weſt
ſouth of *Mendip-hills*, ſo *Cotſwold* to the north-eaſt, and t
chalk-hills of *Marlborough-downs* and *Salisbury-plains*, ſee
to ſet bounds to the coal country, to the eaſt and ſouth-ea
of which Fig. 3. Plate I. may be ſuppos'd a ſection fro
ſouth-eaſt to north-weſt, *viz.* from the dip to the riſe; a
Fig. 4. a ſection at right angles, from ſouth-weſt to north-ea
on the drift or level.

This Mr. *Strachey* mentions by way of correction a
addition to his former obſervations of the coal works in *Some-
ſetſhire*. He ſince had opportunities of being under groun
and viewing ſeveral coal works in *Scotland* and *Northumbe-
land*, and of obſerving the ſeveral *ſtrata* there. At *Widdringt*
they have four fathom clay; then a ſeam of coal about f
inches thick, not worth working; then a white freeſton
then a hard ſtone, they call a whin; then two fathoms
clay; then a white ſoft ſtone; and under that a vein of co
three feet nine inches thick. This is a ſmall coal of the ſa
nature, but not ſo good as the *Newcaſtle* coal, which com
to *London* market. Theſe veins dip to the ſouth-eaſt, o
yard in twenty. Near *Trannent*, in *Eaſt-Lothian* in *Scotla*
the coal alſo dips to the ſouth-eaſt in the ſame proportio
but at *Baldoe*, in the pariſh of *Campſy*, three miles fro
Kylſith, it dips to the north-eaſt; and at *Madeſtone*, ne
Falkirk to the ſame point, and in the ſame proportion. T
ſtrata of earths and minerals, at theſe places, agree ve
nearly; they have, as the ground riſes or falls, one, two
three fathoms of clay; then 11 fathoms of ſlate, or coal-clive
one fathom of limeſtone; under that two fathoms of ſla
earth, and ſtone, and then coal: And all theſe agree in thi
that the pits generally need no timber, and have a goo
roof, which is ſupported by pillars of coal, which they lea
in the working. At *Baldoe* the coal is commonly 45 inche
thick; and all along for ſome miles eaſtwards thence; on th
ſides of the hills are crops of coal and limeſtone; and ofte

time

imes juſt under the ſurface; the tenants ſpit up as much as will ſerve their turn for a winter's burning; for there wants market, and it is ſcarce worth working for ſale. And to he north-weſt and north, in the drift of the coal in higher round, and conſequently, lying over it, there appear in the ides of the hills, ſeams of ſpar and lead, the drift of which s north eaſt, and lies almoſt perpendicular; but what obliquity there is, pitches to the ſouth-eaſt. At *Auchenclaugh*, ix miles eaſt from *Kylſith*, there is a coal 18 foot thick; this lips one foot in three, aud is not purſued by reaſon of water; nd for want of a market, will not quit the coſt of draining. At *Madeſtone*, the coal is four feet and a half thick, upwards f three fathom and a half deep: They land it (as at ſeveral coal-hews in the country) on girls backs. Near *Trannent* here are three different veins work'd; the undermoſt is bout 18 fathoms from the ſurface, call'd the ſplenty coal, our feet and a half thick; it is a hard, but not large coal, makes a clear and ſtrong fire, lies 10 fathom under the main oal, which is nine or ten foot thick, and comes out very arge. Its roof is of freeſtone, under which Mr. *Strachey* walk'd backwards and forwards for two hours; but had no pportunity of making any other obſervation on the upper ein, than that it is about four foot thick, and neither ſo ard or large as the other.

As Fig. 3. and 4. repreſent the different *ſtrata* (which Mr. *Strachey* had an opportunity of obſerving) on a ſuppos'd plane, as they lie there; Fig. 5. and 6. repreſent the ſame in globular projection, ſuppoſing the maſs of the terraqueous globe to conſiſt of the foregoing, or perhaps, of ten thouſand other different minerals, all originally, whilſt in a ſoft and fluid ſtate, tending towards the center: It muſt mechanically and almoſt neceſſarily, follow, by the continual revolution of he crude maſs from weſt to eaſt, like the winding up of a ack, or rolling up the leaves of a paper book, that every one of theſe *ſtrata*, tho' they each reach the center, muſt in ſome place or other, appear to the day; in which caſe there needs no ſpecific gravitation to cauſe the lighteſt to be uppermoſt, *&c.* for, every one in its turn, in ſome place or other of the globe, will be uppermoſt; and were it practicable to ſink to the center of the earth, all the *ſtrata* would be found in every part, and according to the poet *ponderibus liberata ſuis.* Add to this, that in all the places within Mr. *Strachey*'s knowledge Dr. *Stukely*'s obſervation holds good, *viz.* that

H 2 the

the precipices of all hills are to the westward; whereas the ascent to the east is more gradual. The farther enquiry into which, Mr. *Strachey* offers to the curious, who have better opportunity.

The common Experiment for proving French Brandy, *shewn to be both false and fallacious; by M.* Neuman. Phil. Trans. N° 391. p. 398. *Translated from the* Latin.

SOME merchants in *Holland*, *England*, *Hamburg* and *Dantzick*, who chiefly deal in *French* brandy, boast of a certain probatory experiment, as a very curious *arcanum*; they firmly persuade themselves, that by means of this experiment, they can distinguish not only *French* brandy from malt spirits, but likewise what is genuine from adulterated brandy; and that consequently they cannot be imposed upon in buying the genuine sort. Hence it is still looked upon as an undoubted, infallible proof, nay, as some grand secret: So that M. *Neuman* could not procure, either to smell or taste this proof-liquor.

This liquor is of a duskish, yellow tincture, and with it the experiment is made in the following manner; first they fill a glass with the brandy, that is to be prov'd; and into this glass they put 1, 2 or three drops of that liquor, more or less, according to the quantity of brandy therein; and if the brandy be good and genuine, there immediately appears at the bottom of the glass, a very beautiful blue colour, which, if stirr'd, and well mix'd with the rest of the brandy, tinges it entirely of an azure; but if they be malt spirits, there is no such tincture to be seen in the glass, these retaining their pristine tincture, tho' 20 times the number of drops be put into the glass. As, therefore, this method of proving does, according to the opinion of the merchants, generally serve to distinguish pure malt spirits from pure *French* brandy: So, depending on this hypothesis, they judge of the different degrees of the adulteration of brandy, and malt spirits; for, they pretend to judge, from observing the blue colour, unless it be obscure, but provided it be a blueish gray or a blueish green, that the brandy is not only adulterated, but likewise with what proportion of malt spirits, more or less, it is mix'd.

It is true, M. *Neuman* was at first very much pleas'd with this method of proving spirits; so that at first view he took it to be altogether certain and infallible: But considering

with himſelf, that no demonſtrative experiment had hitherto been made, by which we might ſhew, or at leaſt ſuſpect, the peculiar conſtitutive parts of pure *French* brandy, and that highly rectified, diſtinct from malt ſpirits, equally rectified; but that both conſiſt of the ſame eſſential parts; to which common quality they are ſimply reduced by neceſſary fermentation, he, therefore, thus reaſoned with himſelf, that if any remarkable difference happened in the ſaid ſpirits, it was owing, neither to the intimate compoſition that conſtitutes the brandy itſelf, or to the nature of the wine, from which it is diſtill'd, but neceſſarily ariſes from ſome heterogeneous additament, no ways conſtituting brandy, as ſuch; whether it be added in fermentation or diſtillation; or whether it happen by tinging, extracting, or even by the commixture of other liquid or ſoluble bodies.

M. *Neuman* would not acquieſce in ſuch thoughts alone, for the moſt part, precipitate and fallacious; but made ſeveral experiments to that purpoſe; by which he eaſily had a confirmation of his conjecture, namely, that that apparent difference by means of the above experiment, is in no manner a true and eſſential diſtinction of ſpirits; but that the production of the blue tincture, by which the difference is judged, is owing to ſome heterogeneous additament, plainly not requiſite to the peculiar conſtitution of brandy; and conſequently, that the whole experiment, however plauſible it may appear, is falſe, fallacious, and uſeleſs, which he will farther ſhew anon.

M. *Neuman* underſtood by a friend, who had taſted this proof-liquor at *Dantzick*, that it was of a ſtyptic ſavour: Wherefore, directing all his experiments this way, he at length found, that it was no other than a mere ſolution of iron, in a vitriolic acid; whether it conſiſt of iron, diſſolv'd in ſpirit of vitriol, and diluted with water, or of *Engliſh* vitriol, or ſome venereo-martial vitriol, prepared by precipitation, or of an extract of ſome iron-ore or earth, as the *Heſſian*, tho' the moſt elegant blue colour be produced from the laſt mentioned, namely, with the liquor of *Heſſian* earth; and the more ſaturated the ſolution is, the leſs of it is required for making the experiment.

That additament, which firſt communicates a yellow tincture to *French* brandy and then a blue in the courſe of the experiment, is oak-wood, or the ſhavings or chips thereof, infus'd in brandy, or when the brandy is kept in a new oak-

caſk,

cask, till it hath extracted a yellow tincture from the wood; and the more yellow it is, the more blue it becomes by the martial liquor in the experiment, unless it be tinged with saffron, or some other yellow body.

That the whole is owing to nothing other than the oak-wood, M. *Neuman* confirmed by the following experiments; he took malt-spirits, the same, to which when the liquor is put, they neither exhibit a blue tincture, nor undergo any other change; and therefore, reckoned pure malt spirits by the merchants; in these he infus'd oak-chips, till the spirits had been almost as yellow as *French* brandy; and after filtration, he poured into them some of the vitriolic liquor, as is done into *French* brandy, and it produced the same elegant blue tincture, without any the least apparent difference; which abundantly shews, that the entire change is owing to the oak-wood.

In like manner, may any other substance, resembling oak, as for instance, galls, be infus'd in malt spirits; the experiment also succeeds in some measure with pomegranate-rind, and other astringent vegetables; yet best of all with oak, to which the pomegranate-rind is far inferior, as exhibiting rather a violet than a blue colour, and when stirred, appearing somewhat green.

It is worth observing, that a very small quantity of the liquor, extracted from oak, is sufficient to give a blue tincture (as the above-mentioned liquor does) to a large quantity of malt spirits; for, with a single drop of that infusion, M. *Neuman* made the experiment answer in half an ounce of malt spirits.

That the liquor, or solution should consist and be prepar'd of pure iron vitriol, and not of that of copper, appears hence. 1. Because the experiment, made with *Goslar*, *Hungary*, *Dantzick* and all such mix'd vitriols, that contain any copper, more or less, does not succeed so well, and they produce a very diluted blue, or grey tincture. 2. Because the experiment does not at all succeed with pure vitriol of copper, nor strike any blue tincture as M. *Neuman* found by several experiments to this purpose, as shall be shewn anon.

It should still remain to explain the causes, and origin of this blue tincture; but because M. *Neuman* takes the composition to be the same with new or diluted ink, whose chief ingredients, that produce the tincture, are the same, namely, vitriol

vitriol of iron, and an aſtringent vegetable, on which M. *Lemery* has writ a learned and copious diſſertation (vide *Hiſt. de l'Acad. des Sciences pour l'An.* 1707) to him he refers; and as to the optical and other philoſophical reaſons, conſult Mr. *Boyle*, Sir *Iſaac Newton* and other authors who have writ on colours.

In fine, M, *Neuman* briefly obſerves, that no other difference ſhould be ſought for between *French* brandy and malt ſpirits, namely ſuch as are pure and carefully diſtill'd, than the peculiar flavour of *French* brandy; tho' the ſame flavour may be ſeveral ways communicated to malt ſpirits, and they thus adulterated into *French* brandy: So that the moſt ſkilful might take them for genuine brandy, or at leaſt, not for malt ſpirits; whence it appears, that the above-mentioned and other probatory experiments on ſpirits are of no uſe, or at leaſt inſufficient.

Here follows an account of the experiments. I. M. *Neuman* prepar'd eight ſolutions of vitriol, taking for each ſolution two drachms of vitriol and one ounce and a half of common diſtill'd waters. The ſolutions were 1. of *Goſlar* vitriol. 2. *Dantzick* vitriol. 3. *Hungary* vitriol. 4. *Cyprus* vitriol. 5. *Engliſh* vitriol. 6. Of iron prepar'd with oil of vitriol. 7. Of iron obtained by precipitation from venereo-martial vitriol, and 8. A ſolution of *Heſſian* earth; of which laſt he only took two drachms to the above-mentioned quantity of waters. II. He made three infuſions of aſtringent vegetables; to each ounce of the vegetable putting one pound (apothecary's weight) of malt-ſpirits, which received no blue tincture from the proof-liquor: The infuſions were 1. Oak. 2. *Turkiſh* galls. 3. Pomegranate-rind. III. With theſe three well ſaturated infuſions, he made from malt ſpirits three ſorts of adulterated *French* brandy, at leaſt reſembling it in the yellow tincture; and with each eight ounces of malt ſpirits, he mix'd an ounce of the infuſion. IV. He took the common yellow *French* brandy, and alſo common malt ſpirits, newly diſtill'd, and with both, and the ſolutions of vitriol, he ſucceſſively made experiments, in the manner juſt mentioned. V. At each time he pour'd half an ounce of *French* brandy into a clean glaſs, narrowing downwards, and from one to four drops of the ſolution of vitriol, more or leſs, as was found ſufficient to produce the blue tincture; and carefully viewing it he obſerv'd, that the ſaid *French* brandy had a pale blue tincture communicated to it, from the ſolution of *Goſlar*, and that of *Dantzick* vitriol;

vitriol; as alſo a bluiſh tincture from *Hungary* vitriol; from the ſolution of the *Cyprus* vitriol it had not a blue but a greeniſh tincture; from the ſolution of *Engliſh* vitriol and from that of vitriol of iron, prepared with oil of vitriol; and likewiſe, the ſolution of vitriol of iron, made by precipitation, a very elegant tincture; but it had the moſt beautiful of all, from the ſolution of *Heſſian* earth. The reaſon of this diverſity of tinctures has been given above. VI. Moreover, for each probatory experiment he took alſo ½ an ounce of malt ſpirits, and put to them 1, 2, 3, nay 10 drops and upwards of each ſolution of vitriol; yet he could not obſerve the leaſt bluiſh tincture produced. VII. Afterwards for each experiment he took ½ ounce of the adulterated *French* brandy of N° III. and dropping into it, as alſo into common *French* brandy, ſome of each of the ſolutions of vitriol of N° V. the experiments agreed in all reſpects, only that, on account of the combination of the extracts of the various vegetables, there happened ſome, tho' a very inconſiderable, difference, as ſhall appear from what follows. VIII. The malt ſpirits became ſomewhat black, upon dropping into them ſome of the ſolution of *Goſlar* and *Dantzick* vitriol, with an extract of galls; and with the ſolution of *Hungary* vitriol, at firſt a bluiſh tincture, but upon mixing, they entirely loſt it; the ſolution of *Cyprus* vitriol ſtrikes no tincture at all; they had an elegant blue tincture from the ſolution of *Engliſh* vitriol; and from that of iron, both ways prepared, they at firſt, it is true, had a blue tincture, but upon mixing and ſtirring them with a quill, they got a violet tincture; in fine, a ſingle drop of the ſolution of the mineral earth of iron gave them a beautiful blue tincture. IX. Malt-ſpirits, impregnated with an extract of pomegranate rind, and mixed with the ſolutions of vitriol in the order abovementioned, had little or no blue tincture, and in the four former trials they appeared almoſt in the ſame manner, as the abovementioned malt-ſpirits, impregnated with the extract of galls; and in the latter, they immediately appeared of a ſomewhat green tincture, and preſently after ſomewhat reſembling ink. X. On the contrary, malt-ſpirits, with an extract of oak, ſomewhat reſemble *French* brandy; and with the ſolutions of pure martial vitriols, but eſpecially with that from *Heſſian* earth, the moſt beautiful blue is produced. With the firſt four ſolutions of vitriol the appearance was the ſame as in other counterfeited malt-ſpirits, nay as in *French* brandy itſelf. XI. At length being ſufficiently perſuaded, that the experiment ſucceeded very well with oak-

ſhavings,

havings, M. *Neuman* had a mind to try the leaſt quantity of he infuſion of oak requiſite to produce ſome degree of the blue incture: He therefore, tried ſeveral quantities from $\frac{1}{2}$ a drachm o 20, 15 drops and downwards; ſo that one ſingle drop (with a rop at leaſt of the ſolution of the mineral earth of iron) was fficient to impregnate the whole half ounce of malt-ſpirits and produce a blue tincture, tho' not ſo greatly ſaturated.

Fork *put up the* Anus, *and afterwards drawn out thro' the* Buttock; *by Mr.* Robert Payne. Phil. Tranſ. N° 391. p. 408.

AN apprentice to a ſhip-carpenter in *Great Yarmouth*, about 19 years of age, had violent pains in the lower art of the *abdomen*, for ſix or ſeven months; it did not appear be any ſpecies of the colic; he ſometimes made bloody ine, which induced Mr. *Payne* to believe it might be a ſtone the bladder: The patient was very little relieved by phyſic. t length a hard tumour appeared in the left buttock, on or ar the *glutæus maximus*, two or three inches from the verge of e *anus*, ſloping a little upwards. A ſhort time after, he oided purulent matter by the *anus*, and that every day for ſome me: The tumour broke; and Mr. *Payne* ſuſpected a *fiſtula ano*, but could not introduce the probe, by the orifice of the re, into the *rectum*. Shortly after, the prongs of a fork apared thro' the orifice of the ſore, upwards of $\frac{1}{2}$ an inch beond the *cutis*, whereupon the patient's violent pains ceaſed. r. *Payne* divided the fleſh between the prongs, and after that, ade a circular inciſion about the prongs, and ſo with a ſtrong ir of pincers extracted it, handle and all entire, not without eat difficulty. The end of the handle was beſmeared with e excrements; the fork was ſix inches and a half long; the ndle ivory, but tinged with a very dark brown colour; the on part was very black and ſmooth, but not ruſty. The pant was likely to do well in a ſhort time.

The patient told a relation of his how this came about; *viz*, at being coſtive, he put the fork up his fundament, thinking that means to relieve himſelf; but unfortunately it ſlipt up far, that he could not recover it again; and he added, that had no trouble or pain, till a month or more after it was put .

Two Cases of Insects, *voided by the urinary Passage; by Dr.* Turner. Phil. Transf. N° 391. p. 410.

NOV. 24. 1725, a woman came to crave Dr. *Turner*'s advice for a child about 16 months old, bringing with her a worm, which she told him the apothecary had just then extracted out of the child's *penis*; having for several days before laboured under very great uneasiness, which she called convulsions of the bowels; the child was continually drawing up the lower limbs, and straining at both *sphincters*; the urine seemed to pass with difficulty for some days, till at last there came on a total suppression, and the worm advancing, shewed itself at the extremity of the *urethra*. The Dr. examined the insect and found it measured upwards of four inches, resembling the worms usually voided by the *anus*, of the earth-worm kind, but whiter, which made the Dr. think at first, they had been mistaken, and that it had been voided that way. The Dr. having directed what he thought proper, went to the apothecary (who had the worm in his custody) in order to be better informed. He assured the Dr. that when he came to the child, he observed a preternatural body, hanging half an inch out of the *glans*, and lying double in the passage; perceiving it advance farther, he took hold of it, and extracted it with little difficulty.

The summer preceeding that of 1725, a woman shewed the Dr. a maggot with a crusty, red *galea* over the snout, and a forked tail, which she had just then voided by the urinary passage.

An Account of a new Machine, *called the* Marine Surveyor, *contrived for the more correct Mensuration of a Ship's Way in the Sea. than by the* Log; *by Mr.* De Saumarez. Phil. Transf. N° 391. p. 411.

THE *primum mobile*, or soul of this machine is in the form of the letter Y, and made of iron or any other metal; at each end of the lines, which constitute the angle, or upper part of that letter, are two pallets, not much unlike the figure of the log; one of which falls, in the same proportion as the other rises: The falling, or pendent pallet, meeting a resistance from the water, as the ship moves, has, by that means a circular motion under water, which is faster or slower according as the vessel moves; this motion is communicated to a dial within the ship) which is fixed either in the master's cabbin, or in any other proper place) by means of a rope of any convenient

length,

length, faſtened to the tail of the Y, and carried to the dial. The motion being thus communicated to this dial, which has a bell in it, it ſtrikes exactly the geometrical paces, miles, or leagues, which the ſhip has run. Thus, is the ſhip's diſtance obtained; and with equal eaſe may the forces of tides and currents be diſcoved by this inſtrument.

AKCL and BHDI (Fig. 1. Plate 2.) are the pallets, which are worked from the legs DE and CE into the form they appear, to a breadth of about four inches and a half. The length of the pallets BD and AC is 8 inches; the branches or legs DE and CE are each 15 inches and $\frac{1}{2}$ in length and 2 inches in circumference, the diameter of which is about $\frac{2}{3}$ of an inch; and the angle CED, which is contained between them, is 45 degrees. The ſhank EF is of the ſame thickneſs or circumference with CE and DE, and is 27 inches long. At the point F is a ring, where one end of the rope FG is hooked to the machine, the other end G being fixed to the dial, within the ſhip or veſſel. This rope may be about 5 fathoms, more or leſs, according as the dial is fixed high or low, with reſpect to the ſurface of the water.

In the Fig. this machine has but two branches; however, it may be formed of 3, if not 4, and adjuſted to the ſame ſtandard or meaſure: But as 3 or 4 branches would be apt to entangle themſelves in ſea-weeds, and thereby prevent the regular motion of the inſtrument, if not in ſome meaſure impede the ſhip's way, Mr. *De Saumarez* recommends their being made only of two branches, in the manner laid down: For, in his own experiment at ſea, he obſerved thoſe made in this form, ſo far from being choaked by weeds, that if they met with any, they always cleared themſelves, without the trouble of hauling up the engine into the ſhip. This inſtrument may be regulated ſeveral ways; as firſt, by opening or cloſing the angle CED; ſecondly, by lengthening or ſhortening the branches, or turning or bending, more or leſs, the pallets AKCL and BHDI; and ſo in this manner, the machine is brought to what ſtandard or meaſure you pleaſe to make the hydraulic revolution anſwer either to a geometrical pace of 5 feet, or to 10, 12, 14 feet, *&c.* The machines of this kind, he tried at ſea in all ſorts of weather, weighing ſome 4, others 5, and others 6 pounds; their weight not at all affecting the peculiar property of the inſtrument, or hindering the regulation thereof, according to the methods he has laid down. Theſe machines may be made of tin as well as iron, and ſo light as not to weigh above 2 or 3

pounds; which may ſerve for any boat, wherry, barge, &c. without any hindrance to their rowing or ſailing. The manner of fixing them to a ſhip, or boat, is repreſented in Fig. 2.

Mr. *De Saumarez* comes next to explain the 3 ſeveral dials, one of which may be made uſe of with this machine: The firſt dial had 3 index's, one of which marked 10 revolutions of the engine, each revolution 10 foot; ſo that conſequently, the whole round of the circle was 100 foot; as 5 of theſe revolutions made 50 feet, which he reckons to be (or at leaſt ſhould be) the diſtance marked between each knot on the log-line, now in uſe at ſea; by holding the half minute glaſs in one's hand (which is always uſed with the log-line) one may, by inſpection, ſee how many times 50 feet ſhe runs in half a minute, and conſequently, how many miles in an hour, without the trouble of employing 4 or 5 hands, as there generally is, in heaving the log. The ſecond index on this dial marked 100 revolutions, which makes 1000 feet, as the third index did 1000 revolutions, which is equal to 10,000 feet; and then a little bell ſtruck, denoting when the ſhip had ſailed that diſtance, which may be alſo fitted to ſtrike to any another meaſure; his ſecond dial had the circle on its plate, divided into 12 parts; ſo that as the index paſt each diviſion, the ſhip had run one mile; and conſequently 12 miles, when it had meaſured the circumference. On one ſide of this dial he had fixed another plate, which was graduated in ſuch a manner, that by the half minute glaſs, he could alſo, by inſpection, tell what the veſſel run in that ſpace of time, &c. On his third dial he had 3 circles; the firſt was ſo divided as to ſhew, when the ſhip had run 60 leagues; the ſecond ſo contrived, as to ſhew when the ſhip had run the ſame diſtance in miles; and on the third was marked 120 knots: So that computing each knot at 50 feet, the circumference was 6000 feet, which he takes to be the ſtandard of an *Engliſh* maritime mile, or the $\frac{1}{60}$ part of a degree upon the equator; in running which length, his inſtrument has juſt 600 revolutions; to which diſtance a little bell ſtrikes to give notice to the man at the helm, of the diſtance ſailed in that time: Beſides the ſeveral circles on this dial (graduated as has been mentioned) he had alſo 2 plates on each ſide, with 2 circles; one divided into 100 leagues and the other into 300 miles. So that, without hearing the bell ſtrike, to every mile or league, one might at any time ſee by them, what number of miles or leagues the ſhip had run, from the time ſhe had left her port: As to the material within the dial there is little more than common clock-work.

A

As by this machine Mr. *De Saumarez* undertakes to cor-ect the errors of the log, he flatters himſelf, that a compari-on between that inſtrument, and his invention, will not be nacceptable to the curious.

The firſt error he touches on in relation to the log, is in the alf and quarter minute glaſſes; he thinks he may well ffirm, that they are ſeldom or never true; in regard it rely happens, that we can find two to finiſh their courſe the ſame ſpace of time: Yet if they did run their ſand out qually, it is no demonſtration of their truth; ſince two, at are falſe may do the ſame, as well as two that are true. ut admitting, they were never ſo truly made, they are not-ithſtanding ſubject to error; ſince it is but too well known, at dry and wet weather have a great influence on them, hould the half minute glaſs lack but two ſeconds, or be two conds too long, it makes an error of ſome miles in 24 hours. f the log be hove by quarter minute glaſſes, in like man-er defective (which is the general practice when the ſhip as great way) in doubling the knots, the error is likewiſe oubled. Beſides, when the ſhip runs at the rate of eight r nine miles an hour, (and the line is left to run off the eel) it rarely happens, but ſome fathoms are out, before he line can be ſtopp'd; tho' this may be ſmall in the courſe f 24 hours, and therefore not regarded; yet in a long voyage t will make a conſiderable addition to the many errors in the iſtance (which we gain by the log) which added to thoſe of ur judgment, occaſions ſo many that keep journals at ſea, o be aſhore, when they have reckoned themſelves 50, 60, r more leagues from the land; and others to be as many eagues from their port, at the time, when they have expected o make it.

In the marine-ſurveyor it is not ſo; for, this inſtrument equires no glaſſes of any kind: Let the ſhip run faſt or ſlow, t is the ſame; for, it works in proportion, and the bell trikes to every mile accordingly. To evidence the truth of his, Mr. *De Saumarez* mentions an inſtance, *viz.* when he as making his experiments on the canal in St. *James's* park, Dr. *Deſaguliers* and ſeveral other mathematicians, at times, were with him; and they meaſured out a certain diſtance here; upon which he fitted his machine to ſtrike to that iſtance, and accordingly it did ſo. They then alter'd the motion of the boat, and row'd much faſter to the mark than before; however, the bell ſtruck, when they came up to it,

to

to the greateſt exactneſs: And ſuch is the property of this inſtrument, that it may be fitted to ſtrike to miles, leagues, &c. as ſhall be thought proper. This machine is made of materials ſo durable, that one of them ſhall laſt 50 or 60 years; and ſuch is the price, that they will prove as cheap, or cheaper to the government, than the log, which is attended with an expence of ſo many lines, glaſſes, &c. As for the making a trial of this inſtrument, it may be as fully done in the *Channel*, as in an *Eaſt India* voyage: For, if it anſwer to 20, 30, or 40 leagues, the reaſon holds good for as many thouſands.

2. The chief property of the log, is to have it ſwim upright or perpendicular to the plane of the horizon. This is too often wanting in logs; becauſe but few ſeamen examine whether it be ſo or no, and generally take it upon truſt, being ſatisfied, if it weigh a little more at the ſtern than the head. What erroneous reckonings flow from hence is but too evident; for, if the log does not ſwim upright, it will not hold water, nor remain ſteady in the place, where it is hove, ſince the leaſt check of the hand, in veering the line, will make it come up ſeveral feet. This repeated, the errors become fathoms, and perhaps knots, which, how inconſiderable ſoever they may ſeem, are miles and parts of miles, and amount to a good deal in a long voyage.

In anſwer to this, the marine-ſurveyor is of ſuch a property, that there is no neceſſity to take care about its ſwimming; and it is a conſtant truth, peculiar to this inſtrument, that be the ſhip's motion on the water what it will, whether ſhe run one mile faſter or ſlower than another; yet all ſhe runs is exactly mark'd on the ſaid inſtrument, as plainly appears from ſome experiments made by Mr. *De Saumarez* in the river of *Thames*, for obtaining the gradual increaſe and decreaſe of both ebb and flood.

3. The ſtretching and ſhrinking of the log-line is another great error in the uſe of the log; for, when a new line is firſt uſ'd, let it be ever ſo well ſtretch'd upon deck, and meaſur'd as true as poſſible, it ſhrinks conſiderably after wetting; and therefore, if we rely on the line run out for the ſhip's diſtance, we ought to meaſure and alter the knots on it every hour, before we uſe it; but Mr. *De Saumarez* is well aſſur'd, that this is ſeldom done oftner than once a week; and ſometimes not above once or twice in a voyage. What great dependance then is there on a reckoning, kept

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by the log? Since in this caſe the line will ſhrink ſo, as to add miles to the other miſtakes of every 24 hours. Again, when the line is meaſured to its greateſt degree of ſhrinking, it is generally left there; and when, by much uſe, it comes to ſtretch again, it is ſeldom or never mended, tho' it will ſtretch beyond what it firſt ſhrunk. In ſhort, ſuch are the errors incident to the log, that Mr. *De Saumarez* does not wonder at the *Dutch* for preferring their chips, or an irregular pulſe to it; which conjectural reckoning of theirs is obtain'd in the following manner; they fix two marks on the ſide of the ſhip at a certain diſtance, when an experienced perſon, ſtanding at the foremoſt mark, throws a chip over board, and counts the ſeveral beats of his pulſe, during the chip's paſſage from one mark to the other; and thence it is they compute the number of miles the ſhip runs in an hour.

As to the marine-ſurveyor; it is not hove with a line; but tow'd aſtern by a rope; and whether that rope ſtretch or ſhrink (be long or ſhort) it is all one; for, the inſtrument will have the ſame true revolutions. Should it be objected, that it holds water, Mr. *De Saumarez* affirms, from his own experiments of it, that the log, haul'd in from five or ſix knots, is much heavier upon the hand; and that the faſter the ſhip runs, the leſs water this inſtrument holds; becauſe it gives way to the water, and turns quicker; nay, he can venture to ſay, that it is ſo far from being any conſiderable impediment to the ſhip's way, that ſhe does not loſe one mile in 100 by it. But ſhould this inſtrument be introduced into the navy, in caſe of chaſing an enemy, or the like, it may be taken in at any time, and let down at pleaſure.

4. Mr. *De Saumarez* appeals to all ſeamen, if in a moderate gale, when the ſhip runs five or ſix knots, two different perſons, every way qualified, were to heave the log immediately after each other, whether they would exactly agree. Surely not: Since it is but chance, if they do ſo, and is what may not happen in a hundred trials. He, therefore, affirms the log to be very erroneous on this account, and that the error frequently increaſes with the wind; for, in a ſtiff gale, when a ſhip has run about eight or nine knots before the wind, it has been known, that two expert ſeamen have hove the log in this manner; and on their comparing notes, they have found a knot difference; ſometimes it has been more, and at other times leſs, which muſt certainly occaſion a ſtrange confu-

confusion in the reckoning. Under this head Mr. *De Saumarez* observes, that when the log is hove, it is sometimes in so strong a gale, that the ship runs nine knots; but before it is hove again, there may be such a decrease of the wind, that for half of the hour she may not run above five knots. Her true distance sail'd then, is the mean between the extremes of nine and five; but this has been so far from having been considered by some *Chalkers* of the log-board, that it is but too well known, the extremes have been put for the mean, and the contrary. Were there truth in the log, two ships in company would nearly have the same account; but it is otherwise: For, we too often find several leagues difference in reckonings, even on board the same ship. In a word; such errors have been found in the log by some of Mr. *De Saumarez*'s acquaintance, that when they have sail'd between a meridian and a parallel, the whole difference on the log-board has not prov'd difference of latitude enough to agree with their observation; tho' each day they had a good observ'd latitude, and no currents.

In the marine-surveyor we are so assur'd of the ship's distance, that all ships which are in company shall agree, as to their reckonings, only that some allowance is to be made for difference of judgment in the several persons, who keep journals.

There are several other cases equally, if not more momentuous, than what Mr. *De Saumarez* offers here, wherein the marine-surveyor shall be found to have the preference of the log.

Here follow the affidavits of some expert seamen, who made trial of Mr. *De Saumarez*'s instrument, as taken under the seal of the royal court of *Guernsey*.

William Ahier, about 40 years of age, who commanded several privateers in the late war, particularly, that call'd *La Chasse*, of about 150 tun, 16 guns and 140 men, and then master of the ship call'd the *Eagle*, made oath, that on *October* 9, 1720, he parted from *Southampton* with several gentlemen passengers on board, for the island of *Guernsey*; that he had fixed at the stern of his ship, a new invention, call'd the marine-surveyor, projected to the best of his knowledge, by Mr. *Henry De Saumarez*, a gentleman of the island of *Guernsey*, for correcting the log; &c. that after they had left the *Needles*, they had a stiff gale of wind, attended with a rolling sea; notwithstanding which, the machine

machine worked as regularly as if it had been ſmooth water, the little bell of it ſtriking with great exactneſs to every mile the ſhip run; and that having thoroughly viewed and examin'd the experiment of this new invention, he found it to be not only practicable, but preferable to the common methods, uſed at ſea, for finding the ſhip's diſtance ſailed; that therefore for the public good, he atteſts the truth of the abovementioned particulars.

Abraham Le Meſurier, of about 48 years of age, formerly captain of ſeveral ſhips; *Peter Bonany*, of about 58 years of age, formerly captain of ſeveral ſhips, and who had uſed the ſea upwards of 40 years; *John Hardy*, of about 38 years of age, formerly captain of ſeveral ſhips, the aboveſaid *William Abrier*, and *James Hubert* of about 27 years of age, who had alſo been maſter of ſeveral veſſels, made oath, that on *October* 19, 1720. they ſet ſail in the morning out of *Guernſey* pier, with a freſh gale of wind, in a ſloop, called the *Dolphin*, in company with ſeveral Gentlemen of the ſaid iſland, in order to make an experiment at ſea of a machine, called the *marine-ſurveyor*, projected, to the beſt of their knowledge, by Mr. *Henry De Saumarez* of *Guernſey*, which invention is intended to correct the many errors of the log, *&c.* And they farther declare, that they have not only thoroughly viewed, conſidered, and examined the ſaid machine; but alſo made ſeveral experiments of it in a rough ſea, ſometimes ſailing right before the wind, then quartering, and at other times turning to windward, and then lying by, in order to know the drift of the ſhip, both with and againſt the tide; that having tried the ſame invention all manner of ways, they find it much preferable to the log, or any of the methods in uſe for obtaining the ſhip's diſtance run, having nothing to object againſt it, as to its being a clog or hinderance to the ſailing of the ſhip, *&c.* That being fully ſatisfied of the great uſefullneſs of this invention for the improvement of navigation, and the ſervice it may be of to all the maritime powers, they publickly atteſt the truth of the abovementioned particulars.

Here we have ſome proof of the uſefullneſs of this new invention, and that from ſeamen of long ſtanding and practice.

But notwithſtanding theſe teſtimonials, Mr. *De Saumarez* was determined to have it tried ſtill farther: Accordingly, he made a preſent of one of his machines to Capt. *John Thoumes*, who, beſides his knowledge in the theory and practice of navigation, was the better qualified to make trial of it, in regard he had ſometimes accompanied Mr. *De Saumarez* in his experiment on

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the canal in St. *James*'s park and on the river of *Thames*. As he was then going a voyage, Mr. *De Saumarez* intreated him to act impartially, and to lose no opportunity of letting him know, how far, and with what certainty, his invention might be depended on. Agreeable to his request, the Capt. writ twice to Mr. *De Saumarez* on this occasion. His first letter was dated at *Nantes October* 20. 1724, an extract of which is as follows.

'According to my promise, I am to acquaint you, that I 'have had as favourable an opportunity, as I could have wish'd 'for, to try your *marine-surveyor*; for, some part of my voyage 'being from St. *George's channel* to the *Bay* of *Biscay*, I pas'sed close to the *Lands-end* of *England*, with a moderate gale 'of wind at north, our course S. by E. when I had the *Lands-'end* east of me about 3 miles I began to reckon, and next 'morning, when *Ushant* bore west about 5 miles distance, the '*surveyor* had made just 37 leagues. These two noted head-'lands, which are very near under the same meridian, differ in 'latitude about 33 or 34 leagues. As to the tides, we crossed 'them, having in this run two floods and two ebbs; and as the 'wind blew cross the *Channel*, one tide was no more influenced 'by it than the other; nor could the current be any impediment 'to the trial. Now as to our having 3 or 4 leagues more than 'the true distance, the reason is very plain; since it cannot be 'expected, but that a ship before the wind will deviate from 'her true course, sometimes one way, sometimes another, in 'her *yaws* and *sheers*. Of this all seamen are sensible. What 'I would remark from hence is, that the *surveyor* measures all 'the little traverses exactly; it is, therefore, the business of the 'navigator to allow for this, when he works the ship's run. 'But I cannot help observing here, that a good effect is pro-'duced from these little traverses being so measured; for, should 'we be running boldly on the land in a dark night, it forewarns 'us to look out in time, by marking somewhat more than the 'true distance, sailed upon a streight line.

'Many are the advantages which accrue to navigation by 'this invention; which I shall not take upon me to enumerate: 'In short the sailors are in love with it; and, when at the 'helm, they value themselves on chalking more miles than 'those who went before them. For my own part I am so 'pleased with it, that I have done with the log. One excel-'lent quality I observe in it is, that in plying to windward 'along shore in a dark night, our usual way by the log is to 'stand 2 or 3 hours out, and so many in; and here we may be

ashore

ashore before we are aware; because in running out we may not have had so much wind, as in running in; or we may have reefed topsails, shortened sail, hankered in the wind, or have met with several other impediments, which, by being drousy in the night, a man may sometimes not take notice of: But it is otherwise with the *surveyor*; for, if the ship be hindered in her way, it will not mark more miles than she has run. I have shewn it to some curious persons at *Nantes*, who are exceedingly pleased with it.

'*P. S.* When I said my course from the *Lands-end* to *Ushant* was S. by E. it must be understood, that I did not go on the outside, but passed within, between *Ushant* and the main: For, in the other case, to pass to the westward, the course had been about S. by W. to go clear of all.'

The substance of Capt. *Thoumes* second letter, dated at *Guernsey September* 2. 1725, so far as it relates to Mr. *De Saumarez's marine-surveyor*, is, as follows.

'I am now fully persuaded of the usefullness of your *marine-surveyor*, having tried it this last voyage to *Marseilles* and *Toulon*, and that it is greatly preferable to the log.

'Having, in two former voyages in the *Bay* of *Biscay*, been apprized, that the ship's distance sailed, as obtained by the *marine-surveyor*, was really true; yet I was obliged every 24 hours to shorten the distance by a certain proportion, that I conjectured to be near one seventh part of the whole; which from the bearings of headlands, &c. I found constantly so. However to be better satisfied about this allowance, I wanted a long run, near, or upon a meridian, with good observations, which could not be had in the *Bay*, or our *Channels*; when, therefore, I sailed for the *Mediterranean*, which was in *January* before, I continued to make the same allowance, and cautioned my mate to make it so too. It happened, that for the first eight days, we had hard gales of southerly winds, attended with violent squalls of rain, and a distracted sea; insomuch that we tried, under a double reefed mainsail, great part of the time, and drove to the westward without the benefit of celestial observations; yet all the while the *marine-surveyor* struck the miles of our drift, which are to be seen upon our journals for every hour; and so far did I depend upon it, that I did not order the log to be once hove.

'After the bad weather, the wind changed with the new moon, to N. N. E. and N. E. with a brisk gale, which gave us a fair run for 5 days, near 50 leagues every 24 hours. We

'had daily obſervations, and our courſe was near ſouth. Here 'it was, that I found the one ſeventh of the ſhip's diſtance was 'to be deducted from the whole, and that it was for *yaws* and '*ſheers* which the *marine-ſurveyor* marks exactly. After this 'allowance was made, ſo well did my reckoning agree with my 'obſervation, that when there was 2 or 3 miles difference, I ra-'ther imputed it to the want of exactneſs in my obſerving, or a 'fault in the quadrant, than to the *marine-ſurveyor*, in regard 'my mate alſo found it to agree to a ſurpriſing exactneſs.

'Three weeks after our departure, I had the misfortune to loſe 'the fork of the machine; and therefore, was afterwards with-'out the help of the *ſurveyor*, till our arrival at *Toulon*, where 'I in ſome meaſure repaired my loſs, by ordering a ſmith to 'make two ſuch forks, of nearly the ſame dimenſions and turn 'in the fins, as I could remember the other had, which ſerved 'there ſo well, that all were ſurpriſed who ſaw me try it. A 'noted profeſſor of mathematics in the *College* of *Jeſuits* there, 'to whom I ſhewed it, was exceedingly ſurpriſed at the regular 'motion of the machine under water, and more, that it ſhould 'ſo nicely determine the diſtance ſailed of any ſhip or boat. 'Beſides ſeveral other Gentleman of the town were all greatly 'pleaſed with the invention.

'The machine, made by my directions at *Toulon*, I made 'uſe of in my way home, and found it to anſwer very well in 'the ocean: Whence ariſes this remark, which ſufficiently 'ſhews the uſefullneſs of your invention, *viz.* That even 'rough ones, made by a mere cobler of a ſmith, and turned by 'the directions of a ſhort memory, are capable of anſwering 'the end for which you invented them.'

'It muſt be noted, that tho' I allow one ſeventh of the ſhip's 'diſtance for her deviation from her courſe; yet ſome ſhips are 'ſo built, that they will ſteer much truer, and others worſe than 'ours did; and in this caſe the *marine-ſurveyor* ſhews its 'worth; for, if two ſhips are in company, the one ſteering 'well, and the other ill, the latter ſhall have more miles than 'the former, on comparing their run, tho' they ſet out from the 'ſame port, and never part company.'

To the foregoing teſtimonials Mr. *De Saumarez* adds ſome other certificates, whereby it will farther appear, that the *marine-ſurveyor* has the preference of the log, *viz.*

Michael Hales, *Benjamin Hutchinſon*, *Joſiah Hales*, *Peter Perchard* and *Robert Gamble*, maſters of ſhips, *&c.* on *October* 21. 1725, accompanied Mr. *Henry de Saumarez* on board

oard the *Richard Yatch*, in order to make an experiment of n inſtrument, invented by him, called the *marine-ſurveyor*; nd as by it he propoſed to aſcertain the way of a ſhip in the a, much more correctly than by any thing, hitherto invented r that purpoſe, they tried it between *London* and *Graveſend* ith the log (which they hove ſeveral times) to which it apeared to them to have the preference; for, by its conſtant and gular motion, the ſhip's diſtance ſailed muſt be more exactly btained than by the log; which, being hove but once in an our or two, cannot be ſo correct, in regard the wind may increaſe or leſſen ſoon after the log is hove, in ſuch manner, that entirely depends on him who chalks the log-board to allow r it. As therefore, very conſiderable errors muſt thence ariſe, a proper allowance be not made for an increaſe, and decreaſe f wind; and as the *marine-ſurveyor* is not ſubject to this, but eeps a regular motion, according as the wind is more or leſs; hey therefore, are of opinion, that this new invention is not nly an ingenious contrivance in its kind, but exactly calculated r the ends propoſed.

John Harris, who uſed the ſea for 30 years, and who was ately mate of the *William* and *Thomas*, bound from *London* to *anſo* in *America*, was preſent, when Mr. *De Saumarez* came n board his veſſel, and fixed an inſtrument at her ſtern, called he *marine ſurveyor*, invented by him for aſcertaining the way f a ſhip in the ſea, much more correctly than by the log, or ny method hitherto in uſe for that purpoſe; and as he deſired s to try it with the log, and to make an impartial report, wheher we found it preferable to the log, or not; we accordingly ept our reckoning both by the log, and this inſtrument, and ound it much preferable to the log, or any thing that had yet ppeared to us for obtaining the ſhip's diſtance ſailed.

Mr. *De Saumarez* having deſired *Thomas Picot*, Capt. of he *William* and *Thomas*, to try his inſtrument with the log, nd impartially report to him, whether he found it preferable, r not to that method of obtaining the ſhip's diſtance ſailed, he rit him a letter dated *Nov.* 16. 1725, from the iſland of *uernſey*, the ſubſtance of which is as follows.

That he had made uſe of the *marine-ſurveyor* in his voyage o *Canſo* in *America*, and had been more than ordinarily careful herein, in order to make a juſt report of it; that he had tried t upon a meridian with good obſervations, and found it to anſwer his expectation, and to be preferable to the log, particuarly in rough and ſtormy weather; that it had been much admired

admired by ſeveral maſters of ſhips and particularly, by Capt. St. *Loe* of his Majeſty's ſhip the *Ludlow-caſtle*, who expreſſed a great liking to it. He concludes his letter, with wiſhing Mr. *De Saumarez* had an opportunity of peruſing his journals, whereby it would fully appear, how much his invention is preferable to the log.

Capt. *Henry Daniel*, coming over as a paſſenger from *Canſo* to *England*, in the aforeſaid veſſel *William* and *Thomas* ſent Mr. *De Saumarez* the following certificate, dated *Dec.* 4. 1725, *viz.* that having been at ſea upwards of 12 years, firſt as a volunteer, and afterwards as a midſhipman, he lately came over as a paſſenger in the *William* and *Thomas* from *Canſo* to *Plymouth*, in which veſſel there was an inſtrument, fixed at her ſtern, called the *marine ſurveyor*, invented by Mr. *De Saumarez*, for aſcertaining the way of a ſhip in the ſea; and that we found it much more correct than the log; and that in a gale of wind, our reckoning by it agreed with our obſervation; which the reckoning by the log ſeldom did; and that we kept our reckoning both by his inſtrument and the log, and found it much preferable thereto, or to any other method for obtaining the ſhip's diſtance.

Of fixed Alkaline Salts; *by M.* Charles Neuman. Phil. Tranſ. N° 392. p. 3. *Tranſlated from the* Latin.

THE mixed ſalts, which are at preſent to be conſidered, are commonly called, alkaline, fixed, or lixivious ſalts. 1. They are called lixivious ſalts, becauſe obtained by elixiviation. And 2. fixed ſalts, becauſe they are neither rendered volatile, nor ſublimed by the greateſt degree of fire. 3. What is meant by an *alkali*, is alſo well underſtood, namely, what is oppoſite to an acid, or which is the ſame thing, an *ant-acid*; the term is from the *Arabic alcali*, *kali* ſignifying ſalt, to which *al* is prefixed, in the ſame manner as in *Engliſh* the definite article *the*; conſequently, *al kali* denotes *the ſalt*. Beſides, it is well known, that a certain herb, called *kali*, and by *Lobelius ſoda*, is found growing in places on the ſea-ſhore, and to abound with a ſalt; this *kali*, burnt and made into a lye, uſually yields a large quantity of ſalt, formerly ſimply called *ſal alcali*: And ſince this very ſalt, probably, gave the hint of burning other plants and vegetables, and that the ſalts obtained from them were found to be almoſt of the ſame quality and nature, it is very credible, that the ancients, on that account, retained the original name equally for both theſe kinds, and probably, only

in

process of time added this or that name of distinction; so
at besides pure alkaline salts, there are also earthy *alkali's*;
the former (which M. *Neuman* considers at present) they
dded the name of *sal*, to distinguish them from the earthy
lkali's; and being soluble in water, they call'd them a mix'd
l alcali, or in general, *salia alcalia*; lest, probably, the
ord *alcali* alone should become equivocal, or occasion any
mbiguity between a saline and earthy *alcali*.

In general, it is to be noted as to the appellation, that it is
ot sufficiently expressive, whether these salts be called
caline only, or only fix'd salts; because both these deno-
inations may still suggest an equivocal meaning: For, first
e have both volatile alcaline salts, and fix'd salts, which
e not pure alcaline salts, but constitute what are call'd
eutral salts: Wherefore, it will be proper to call them by
oth names, namely alcaline fix'd salts.

In other respects, it is sufficient for us to know, that by the
ord *alcali* is meant an ant-acid, and by *fix'd*, something that
n stand the fire, and which, by the greatest degree thereof
nnot be entirely destroyed or carried off; and that for
veral ages backwards down to this very day, salts of this
ind (by way of distinction in chemistry) have been so call'd,
distinguish them from other salts: In fine, it is of little
portance to a practical chemist, whether words of this kind
e of an *Arabic*, *Chaldaic*, or other original, provided he can
eak or write his mind, so as to be understood by those
nversant in the art.

The original *alcali* is still to this day got, not only from
e plant *kali* or *soda* alone, but likewise from other plants,
at grow by the sea-shore, and that are not destitute of salt.
ut as *soda* or *kali*, probably yields the greatest quantity of
is salt, it, therefore, is call'd *soda*; and as it grows and is
repared in very great quantities in *Spain*, it is exported,
ld and made use of under the name of *Spanish soda*.

But as this *kali* participates considerably of sea-salt, with
hich, as has been said, other herbs are mix'd; and the
hole incineration and subsequent preparation is not so care-
lly gone thro', but only perform'd by the common people
e natives; it is plain, that this *alkali* or *soda* does not
eld a pure chemical alcaline salt, but is adulterated with
her salts and impurities; as also with a great deal of earth;
r which reason it is not now made use of either in chemistry
medicine, but only by glass-makers, soap-boilers and

dyers:

dyers: But ſhould any one put it to chemical uſes, it muſt b
depurated and ſeparated from all the impure and adventitiou
things, which have either been mix'd with, or accidentall
introduced into it. So much for the original of the wor
alkali; now to return.

A pure fix'd alkaline ſalt muſt neceſſarily have the fou
following properties of every *alkali* in general, and of whic
alkaline volatile ſalts are alſo poſſeſſed. 1. To cauſe an effe
veſcence with any acid. 2. To change into a mean ſalt wit
theſe acids. 3. To precipitate all ſolutions, made with acid
4. To change the blue tincture of the ſyrup of violets into
green. An alkaline fix'd ſalt, on the contrary, entirely diffe
from a volatile alkaline ſalt in the following properties. *viz*
1. That it precipitates a ſolution of ſublimate, not into
white, which a volatile alkaline ſalt does, but into a duſki
citron colour. 2. That upon putting copper to it, it does no
produce the leaſt blue tincture. 3. That it does not cauſe an
ſmoke with ſpirit of nitre. 4. That mix'd with water, it doe
not cool, but rather warm it. 5. That upon expoſing it to th
air, it becomes moiſt, and at length diſſolves. 6. That i
does not fly off in the fire, but continues fix'd, and loſes almo
nothing of its weight in a gentle glowing fire, and but ver
little in a ſtrong fuſing one. 7. That it not only remains fix'
in the fire, but likewiſe is ſoon fus'd; and in fine, 8. That i
does not change into cryſtals, as ſome volatile ſalts do. Beſide
a pure fix'd alcaline ſalt ſhould likewiſe have, and upon tria
diſcover the following peculiar qualities. 1. That in the form
of a ſolution it ſhould quietly incorporate without any altera
tion, or precipitation with the other very pure fix'd alkalin
ſalt, about which we are employed. 2. That it ſhould entirel
diſſolve ſulphur both in the moiſt, as well as dry way
3. When mix'd with a pure vitriolic acid, and reduced into
mean cryſtalline ſalt, it ſhould loſe nothing of its weight, no
in any manner be put into fuſion. In fine, 4. Such a pur
genuine, fix'd alkaline ſalt, beſides all the other abovementione
15 properties, ſhould alſo make a ſapo with greaſe or expreſs'
oils: And if it ſhould fail only in one of the ſaid peculiar pro
perties, then it is altogether an infallible ſign, that the fix'
alkaline ſalt was not pure.

Nature, ſo far as M. *Neuman* knows, affords no fix'd alka
line ſalt, that is ſufficiently pure, and endu'd with the abov
recited properties, but it ſolely exiſts in that common or ſe
ſalt, mix'd with an acid ſalt. This fix'd alkaline ſalt, 1. I

o where, and in no manner found in its pure alkaline state, ut mix'd every where, either with the acid of common salt, or ven with a vitriolic acid, as in some mineral springs. 2. That I. *Neuman* having at length by various methods separated and ewn this alkaline substance, which is to be found in common lt, yet he did not find it perfectly pure and agreeing with e above recited 16 properties, but differing very much from e common vegetable fixed *alkali*. Whence he concludes in eneral, that nature affords no such pure fixed alkaline salt; and nsequently, that all pure alkaline salts are the productions of rt.

If not all, yet the greatest part of these salts, are produced om the vegetable kingdom, or from such natural bodies as are ckoned of that class, from which they may be obtained very large quantities; it may sometimes be shewn in the anial kingdom, yet excepting that in urine and excrement, hich depends on the said common salt, it is in such small uantity, that it does not deserve to be taken notice of. The eason why, it produces little or non eat all of the fixed alkaline lt shall be briefly shewn anon.

It seems however surprising, when we talk of the origin and xistence of alkaline salt, that it is produced from something, at did not contain a single grain of fixed alkaline salt before; from all those things, from which several, nay, hundreds of ounds of fixed alkaline salt are obtained; while before incineation, as to sense there could not be got in all the trials made erewith so much as a single grain of pure *alkali*; and if any lt be discovered, it rather inclines to an acid than an alkaline ature.

The chief and most necessary instrument for producing a xed alkaline salt is fire, in which respect art has the advange of nature, whilst it can at pleasure either diminish or augent it, and thereby resolve and compound various bodies; nd when some nominal chemists have happened to hear that xed alkaline salts may be produced by fire, they have immeiately taken the bare instrument, either for the matter of alkaine salt, or for its constitutive principle, and ascribed the entire ormation of a fixed alkaline salt to the particles of fire, tho' aily experience plainly shews the contrary, and that fire only o-operates as an instrument towards its formation, as shall be rther explained anon.

The subject, from which a fixed alkaline salt should be prouced, must needs contain two very necessary requisites or

 ingre-

ingredients, namely, 1. That it be inflammable and conſiſt of oily, or bituminous particles, and 2. That it be likewiſe endued with an acid ſalt. If therefore, theſe abſolutely neceſſary ingredients be found in the ſame ſubject, it will be ſufficient to yield a fixed alkaline ſalt; whence almoſt all vegetables, or their pure parts, are very apt to produce it; but when one of theſe requiſites is wanting, there will little or no fixed alkaline ſalt be obtained from them. On this foundation, therefore, does the production of all fixed alkaline ſalts depend, and according to the diverſity of the proportion of the aboveſaid two neceſſary ingredients, ſhall this or that ſubject yield, ſometimes more, ſometimes leſs, at one time a weaker, and at another time a ſtronger, or ſome other way differing fixed alkaline ſalt; a large quantity of the oily ingredient alone is not ſufficient, in regard an acid ſalt is eſpecially neceſſary. If, therefore, M. *Neuman* had a mind to obtain a fixed alkaline ſalt from ſome vegetable, either an herb, root, wood, or bark, or whatever elſe it be, he is before hand ſufficiently inſtructed by former trials, from which only he can conclude, what proportion of oil and acid ſalt is contained in the ſubject, and he can likewiſe form a judgment before hand, whether it ſhall yield more or leſs of a fixed alkaline ſalt: For, if it contain but little or no oil, it will yield but a ſmall proportion of alkaline ſalt; and if it contain little or no acid ſalt, or if the acid be volatile, it will yield much leſs ſtill: Whence it is commonly owing to this latter circumſtance, that oftentimes there is little alkaline ſalt obtained from a large quantity of wood, or other parts of vegetables; and as it is unqueſtionable, that every ſound vegetable contains inflammable particles; ſo on the contrary, many of them contain either little ſalt, or even a volatile acid ſalt, that commonly flies off in calcination. There may likewiſe be plants, woods, roots, &c. very much abounding with ſalt and oil, but which, while they ſhould be calcined, only emit a ſmoke, and burn to coals, but not to aſhes, becauſe the air is too much excluded; in which caſe almoſt all the acid ſalt (tho' they may contain a large quantity thereof, eſpecially if the vegetable be ſtill green and not fully dry) gradually evaporates and very little remains, and what does remain yields but a ſmall quantity of aſhes, and ſtill a much ſmaller quantity of fixed alkaline ſalt. This may be demonſtrated, for inſtance, in pine, or any other wood we commonly uſe to burn: If we take ſome hundred weight of that wood, and burn them ſlowly in the open air, we ſhall obtain a ſufficient quantity of aſhes, and

likewiſe

likewise from these a good deal of salt. But if just the same quantity of the same wood be piled up in small heaps, as in charring coals, or in any other manner; and only so much air be admitted, as that the wood may smoke, we shall soon obtain the usual coals, but in such a manner, that no burning coals shall remain, which, when burnt in the open air, yield a smoaking flame; and if afterwards you reduce the said pure coals to ashes in the open air, and weigh them, and compare their quantity with the former ashes, and at last in the usual manner separate the alkaline salt by making it into a *lixivium*, and then weigh them, this latter method shall yield a vastly smaller quantity of ashes and salt with respect to the former, in which the wood was calcined in the open air. This diversity of weight in the same quantity of wood is owing to nothing else, but that in charring the coals, the greatest part of the acid salt, or the spirit of the wood, as it is commonly called, evaporates, which yet is absolutely necessary, and the primary ingredient in constituting a fixed alkaline salt.

It is worthy consideration, that in order to obtain vegetable ashes the free admission of the air is absolutely necessary; for, without it ashes cannot by any means be obtained; whence coals, upon excluding the air, tho' they be kept in a strong and constant fire for some years, always remain such, and never are reduced to ashes. It is no less worth taking notice, that there is so great a difference between the coals and ashes of the same wood. 1. Coals, made into a *lixivium*, tho' with never so large a quantity of water, afford not the least quantity of alkaline salt. 2. No acid, no not the strongest, has any effect on the coals. 3. No coals mix with glass; and yet all these coals yield ashes.

But to return to the formation of an alkaline salt, or the necessary constitutive parts thereof, the truth of M. *Neuman*'s position does farther sufficiently appear from several particular circumstances. 1. Take such rotten wood, as when sound, is wont to yield a pretty large quantity of lixivious salt; burn, make it into a *lixivium*, and proceed with it in all respects, as if it were sound wood; and then at length compute, how little, if any, fixed alkaline salt it shall yield. The reason is plain, because, the oily, and especially the acid saline particles, absolutely necessary to its formation, evaporated, and disappeared. 2. Take a fresh plant, which nature has manifestly endued with a sufficient quantity of salt, separate the acid salt, or the essential salt, as it is called, from it, after which dry the herb

(after first separating the said acid salt) burn and make it into a lye with all possible care, then it will plainly appear, how little, if any, fixed alkaline salt, it shall yield. 3. Taking the same herb, and in the same quantity, yet without separating the acid, or essential salt, only drying, burning and making it into a lye in the usual manner, it will manifestly shew the truth of this by the increase of the quantity of the fixed alkaline salt in comparison of the former operation. 4. Or take the herb before extracting the essential acid salt therefrom, calcine it with all proper care, then the same thing will appear, and likewise from this, that such essential salts are not pure acid salts, but likewise manifestly endued with oily particles, and such as are necessary to the formation of an *alkali*. 5. If besides, the same quantity of the same fresh herb be taken, which usually yields a pretty large quantity of alkaline salt, and after weighing the whole quantity, it be divided exactly in two parts, and one half be dried, burnt, and a fixed alkaline salt made from it in the usual manner, which weigh and lay by; but let the other half be laid up to rot and be properly treated, till it entirely putrify, then distil it: Thus we shall obtain, as is well known, an urinous spirit, volatile salt, and empyreumatic oil. But afterwards mark how much fixed alkaline salt can be extracted from the remainder, and tho' made into a *lixivium* with the utmost care, it shall yield very little, and often none at all: the reason is plain, because the acid salt and oil, which are the necessary ingredients towards the formation of a fixed alkaline salt, having undergone a change during the putrifaction, and being partly turned into a volatile salt, or urinous spirit, and partly likewise, and that for the most part, separated hence by distillation into an oil both as to substance and form. 6. If you have pure lemon juice, which manifestly contains both an acid salt, or some few oily particles, or such as participate of an inflammable principle, and take a pretty large quantity of it, as 20, 30 gallons and upwards, and evaporating the watery particles, inspissate, burn and make a *lixivium* of it; thence likewise the formation of a fixed alkaline salt will become manifest. 7. This very evidently appears to our senses in tartar, or even its crystals, which, as is known, contains nothing other than an acid salt, impregnated with a great many oily particles; if therefore, these two primary ingredients, conjoined together, namely an acid salt and oil, be calcined, by means of that necessary instrument, fire, then no more pure acid salt is obtained from the remainder, but on the contrary an alkaline salt is pro-

duced

uced a-freſh, and that in a large quantity: And thus the ſame hing may be demonſtrated of all other ſubjects.

That an oily ſubſtance does likewiſe contribute not a little of s oil, may, among other proofs, be collected from this ſingle cirumſtance, which is obvious to ſenſe; if, for inſtance, four ounds of dried wormwood be calcined, and a fixed ſalt be obained from the lye, we ſhall have about two ounces and a half f fixed alkaline ſalt: Again take four pounds of the ſame ormwood, bruiſe and cut it ſmall, and let its oily, bituminous, r reſinous part be extracted by highly rectified ſpirits of wine, s long as the ſpirits are tinged therewith, then you ſhall exact a great deal, but not all the inflammable parts contained herein; yet ſtill all the ſaline parts ſhall be left behind; and et what remains be dried, and burnt, and let the ſalt got from hence by a *lixivium*, be weighed, and inſtead of two ounces nd a half as before, you ſhall only have one ounce of ſalt: Whence, therefore, it plainly appears, that the abſolutely neeſſary, oily or bituminous ingredient, remaining in the former alcination, contributes to produce a larger quantity of fixed alaline ſalt, but in the counter-proof it is ſeparated by the ſpirits. For farther proof, let what remains after the ſpirituous extract e boiled in water, and then the reſt of the herb dried, burnt, nd made into a *lixivium*, and its ſalt collected, you ſhall only ave 19 or 20 grains; the reaſon is, becauſe, almoſt all the ily and acid ſalt, requiſite to the compoſition and formation f fixed alkaline ſalt, were extracted.

Since, therefore, it is ſufficiently evident, that an acid and a ertain inflammable principle are, as has been already proved, bſolutely neceſſary, to the formation of a fixed alkaline ſalt; ence in the firſt place it may be eaſily gathered, that ſomewhat f theſe two abſolutely neceſſary ingredients ſhould remain in t, as there really does. Fire, as an inſtrument in the formation f alkaline ſalt, does no more than 1. Separate all the ſuperfluous phlegm, 2. All the ſuperfluous acid ſalt in the form of ſpirit, and 3. The volatile and ſuperfluous oil. As therefore, ll ſalts, either acid, alkaline, or of any other denomination, as lſo ſulphurs have earth for their baſis; which earth becomes bvious to ſenſe, the more it is freed from its volatile particles, nd on the contrary reduced to a ſtill more fix'd ſtate: So this may likewiſe be obſerved here in the production or compoſition f fixed alkaline ſalt; for, while the fire does gradually drive away ſome of the more volatile ſulphur and acid ſalt, together with the phlegm, and at the ſame time what remains becomes

more

more fixed, and both the acid and bituminous salt are more concentred, when the phlegm, necessary to the composition of an acid salt, gradually evaporates, which, by means of fire, is freed from the acid salt, and made to exhale; at the same time the earth, which was in the acid salt, is separated; and the longer the fire is continued, or the subject calcined, so much the more of the subtile phlegm, subtile acid and sulphur is evaporated, and so much the more earth is obtained; or so much the more easily does the acid salt together with the small inflammable portion degenerate into earth: Whence the greatest mass of fixed salt consists of pure earth. But because this earth is derived from an acid salt, and so to speak, is of an acid nature, it is impossible, that at the first incineration or calcination, all the acid salt should be freed from it; but that the last be rather more concentred and in respect of the former, which was much more volatile, be now very much fix'd, so that the more fixed salt of the inflammable substance, may remain wedged in each particle of this earth: It is owing to this salt that all this earth retains a disposition of solubility in water, and that little, derived from the inflammable principle, according to the proportion of its commixture, is likewise the cause of its caustic quality.

It is also from the same foundation, namely, because this earth is of an acid nature, that it has a constant propensity of reassuming the acid salt, which is so much the more confirmed by their affinity and intimate commixture with all acids, as also by the magnetism with which fixed alkaline salts do likewise attract an acid from the air. This small remaining concentred quantity of fixed acid salt, as also this earth, that inclines to a saline commixture, and that is now freed from all phlegm, is manifestly the sole cause, that these mixt bodies attract the phlegm, they had lost by the fire: Thence arises that quick and instantaneous extraction of the phlegm, and that solution *per deliquium*, as also that incalescence, after a strong calcination, upon their first feeling the moisture, and even an accension, if any mineral substance be at the same time applied to it, as may in some measure be observed in the caustic salt of tartar, prepared with the martial *regulus* of antimony, as also in caustics. But if alkaline salts be entirely divested of this small quantity of acid and inflammable salt, then there only remains bare earth, which cannot any longer be dissolved in water, nor attract either water or an acid from the air, nor produce any degree of heat, but in all respects appear effete; tho' a chemist may

y at pleaſure revive it again, and reduce it to a diſpoſition ſolution and commixture with water, nay, to the greateſt part its former qualities.

M. *Neuman* fully demonſtrates all that he has ſaid above, ncerning the formation of a fix'd alkaline ſalt, by one gle inſtance, that has already been hinted at, and which y perſon may for his own conviction imitate: Take the ential ſalt of *trifolium acetoſum*, which manifeſtly contains acid ſalt, tho' not pure, but mix'd with oily particles: If, inſtance, one ounce of this be taken, and mixed with ater, it entirely diſſolves therein, as it was before concreted o cryſtals after ſuch a ſolution; and not the leaſt part mains undiſſolv'd or unmix'd with the water; ſo that ither oil, earth, alkaline ſalt, or any other adventitious ing can be obſerv'd, but a pure ſolution of acid ſalt: Again, ke an ounce of the ſame dry and cryſtalline ſalt, put it into glaſs retort, and carefully diſtil it with a gradual fire, en all that M. *Neuman* has mentioned above, of the pro- ction of an alkaline ſalt, will gradually happen, and that means of the inſtrument, fire, which cauſes nothing elſe t an incaleſcence of the retort and of the ſalt therein con- in'd, 1. The ſuperfluous matter, that before lay conceal'd the dry ſalt, as the phlegm and oil, together with the very btile and volatile acid, will be got into the receiver, 2. On e contrary, a good deal of earth will again be ſeparated from , and the remainder, into which ſome concentred ſalt and a ore fix'd oil had inſinuated itſelf, will degenerate into a per- ct fix'd alkaline ſalt; and you ſhall have about three drachms phlegm, impregnated with ſome acid volatile ſalt, which quor may otherwiſe be call'd the acid ſpirit of the plant; d perhaps a ſcruple of empyreumatic oil, and the *caput ortuum* remaining in the retort, ſhall weigh about four rachms, 20 grains and upwards, from which ſhall be btain'd above three drachms of a true, dry, fix'd, alkaline lt; ſo that only a few grains in all ſhall be loſt, of the hole ounce: But, beſides the alkaline ſalt, the phlegm, hich at firſt evaporated, and the ſubtile acid, there is alſo n oil, and a *caput mortuum*, which two laſt will by no means ix with water, tho' all of them would before diſſolve therein, hilſt they ſtill conſtituted the acid oily ſalt; the fix'd lkaline ſalt may likewiſe be deſtroyed, and that ſmall por- ion of an acid and inflammable ſubſtance ſeparated, and ence the quantity of earth be increas'd; and thus the cer- tainty

tainty of all these things be discovered by this single experiment.

A fix'd alkaline salt does, therefore, consist of a certain kind of soluble earth, into which, by means of fire, somewhat of the most fix'd acid salt has insinuated, as also somewhat of the more fix'd bituminous, or inflammable principle, which inclines more to an earthy nature; so as to constitute that new saline mixture, chemists call a fix'd alkaline salt. M. *Homberg*, tho' otherwise a skilful chemist, was mistaken in this, namely, as he only owned an acid salt, that insinuated into the earthy particles, and so constituted a fix'd alkaline salt, without mentioning an inflammable principle. M. *Geoffroy* the elder, and M. *Lemery* have thus far observ'd in their processes, that something else besides the above-mentioned acid salt and earth, *viz.* something of an inflammable principle is necessary to constitute an alkaline salt; but they again have been mistaken in ascribing this to the instrument, fire; since in that vegetable subject, made use of for obtaining a fix'd alkaline salt, a real and substantial oily, or bituminous ingredient is obvious to every one. Let such a subject be put into their hands, in which no actual inflammable parts could be discover'd or suppos'd, but only an acid salt and earthy parts; they may treat this subject in a retort, for upwards of 20 years, with several hundred weight of particles of fire and light, applied to the outside of the retort, or in what manner they please; yet with this express precaution that they do not add the ashes of these matters, which were made use of as instruments, it is absolutely certain, that they cannot obtain one single grain of fix'd alkaline salt: Now, if, according to them, fire were its material constitutive ingredient, they must necessarily obtain it, even from such subjects, as have no essential inflammable particles mix'd with them: And if they cannot actually effect it, as they really cannot, but a matter, which of itself is inflammable, must be assumed; why we are not to regard what is evidently certain and demonstrative, as the cause; or why we should give in to the chimera's of this or that philosopher or chemist, who, perhaps, has not all his life time gone thro' a single chemical process, and judges of chemical subjects and histories not from their intimate qualities, and their essential constitutive parts; but only by means of his circle, line and microscope, or even the chimera's of his own brain, M. *Neuman* does not see.

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That fire is neceſſary, only as an inſtrument, to produce fix'd alkaline ſalt, is likewiſe evident, amongſt other things, om tartar, which poſſeſſes, as is well known, the two imary ingredients, *viz*, oleoſo-bituminous particles, as alſo mani eſt acid ſalt, both which it contains in a large quan- y: But if an alkaline ſalt could be produced from this by t, without fire, then nothing elſe ſhould be made uſe of, t ſome earth, into which a little acid and inflammable bſtance had inſinuated; conſequently, they might for this rpoſe make uſe of crabs-eyes, or ſome other earthy body, hich at leaſt is not impregnated with any other acid; iſſolve, therefore, in a ſolution of tartar, ſo much of the id earthy body, as can be diſſolv'd, evaporate the phlegm nd dry it; then indeed, according to M. *Neuman*'s own ypotheſis, *viz.* that an acid ſalt and inflammable principle, ncentred in the earth, ſhall yield a fix'd alkaline ſalt, con- ſting of a greater quantity of earth and a ſmaller quantity of n acid ſalt and of a more fix'd inflammable ſubſtance, there hall plainly be obtained a certain mix'd body, that in ſeveral ircumſtances reſembles a fix'd *akali*; but let it be farther ied, according to all the proofs of a true and perfect fix'd lkaline ſalt, above alledged, then it will appear, that it ſtill ants ſeveral things, and how the whole mix'd body is only ſolution of crabs-eyes in the acid of tartar, and ſo, that the uperfluous acid and oil, and the crude tartar itſelf are entan- led in it; conſequently, that it by no means conſtitutes a rue and perfect fix'd alkaline ſalt, and upon no other account, ut that there was no fire made uſe of, not as if a great many articles of fire were ſtill neceſſary to it; ſince, as has been aid, the ſuperfluous inflammable parts ſtill remain in the oil f tartar; but becauſe the ſuperfluous oil and acid ſhould e ſeparated therefrom; whence, likewiſe by this experi- ment, the formation of alkaline ſalt is obvious to the eye, rovided only that more of the acid and *alkali* be gradually eparated by the fire, and the operation be compleated by he neceſſary inſtrument. The indefatigable *Kunckelius* ound, that an alkaline ſalt might be ſhewn from tartar and quick-lime, when made into a lye, and that without making uſe of a dry ignition, but ſince it almoſt agrees with the pre- ceeding, and that on the ſame account ſome particles of the mpyreumatic oil of tartar ſtill remains therein, M. *Neuman* thinks it needleſs to bring it as a proof of his hypotheſis; and he might adduce ſeveral other earths or compleat ſolutions of

earth, impregnated with various acids, as false fix'd alkaline salts; but to proceed.

It is very well known, that an alkaline salt can be got from all vegetables, or rather from their ashes; as also that lixivious salts of this kind have for some time been produced from a great many herbs and several other things; and in fine, that all of them are obtain'd in the same manner; nevertheless, M. *Neuman* proposes to give a brief account, both of the fix'd salts of herbs in general, and of the remaining fix'd alkaline salts in particular, usual in chemistry; or rather their preparations.

The chemical process is divided by some into nine, by others into six parts; such as divide it into nine, call the operations on a vegetable subject, as follows, deficcation, combustion, extraction, separation, inspissation, calcination, solution, filtration, and inspissation. But such as reckon only six operations call them, deficcation, combustion, extraction, inspissation and depuration. M. *Neuman* reduces them to these four, *viz.* incineration, elixiviation, inspissation and depuration, while one always presupposes the other: For, if he speak of incineration, that is, burning the vegetable to ashes, this presupposes drying, or that it should be dried before, if not entirely, yet at least a little. And if he speak of the second part, *viz.* elixiviation, every body knows, that no *lixivium* can be made without water, or extraction; as also that this extraction, before it can be call'd a *lixivium*, must be separated from the ashes: So that elixiviation presupposes both extraction and separation. And in like manner in depurations, the necessary calcinations, solutions, filtrations, and inspissations are presupposed.

The whole process of producing fix'd alkaline salts from herbs, consists of the following particulars; take an herb of this kind, and dry it gently, kindle some part of it at another fire, and gradually throw the rest of the herbs into it; so as that they may be burnt and calcin'd by a gentle coal fire only, and not in an open flame; to which this contributes very much, namely, if about the latter end of the operation these coals be gather'd closer together: When the burning is quite over, let the ashes cool, sift them, in order to separate the particles that are not quite calcin'd; make a compleat *lixivium* of the ashes in cold water, filtre the *lixivium*, and let it evaporate gently in an earthen or glass vessel, and in fine let it inspissate to dryness. If the salts be design'd for

medical

dical uſes, this firſt inſpiſſation may be ſufficient; but if y muſt be depurated, nay partly cryſtalliz'd, then they calcin'd a little, and afterwards diſſolv'd in a ſufficient antity of water, filtred, and then gently evaporated, till a all pellicle begin to appear on the ſurface; after that pour nto a truncated cucurbite or other glaſs of a large orifice, ntly covering it with paper to preſerve it from aſhes or er filth, and lay it by in a moderately warm place, then reat part of it will gradually turn into beautiful cryſtals, ecially if the *lixivium*, before the pellicle begin to appear, taken from the fire, and put by in a place moderately rm to cryſtallize, and lie there a ſufficient time without rring it. Then after it has ſhot into cryſtals, decant the maining *lixivium* from them, and again evaporate it at a ntle fire, and put it in the ſame warm place, that the mainder may cryſtallize, as much as poſſible; yet the ſtals commonly are not near ſo beautiful, as the former, t are wont to be much ſmaller, nay, form'd after a quite ferent manner; then decant the remaining liquor, and at gth let it be fully inſpiſſated; let the cryſtals be quickly ſh'd with a little common diſtill'd waters, and gently ied from any remaining moiſture, upon a piece of clean tting paper. If you would only have the depurated ite alkaline ſalt, but not the cryſtals, you may ſuperſede above-mentioned tedious proceſs, and after the firſt cal-ation, ſolution and filtration, only inſpiſſate it gently to yneſs, and then put it up in a cloſe veſſel from air. This, n is the general method of preparing the lixivious ſalts of rbs, flowers, ſtalks, *&c.* in the beſt and pureſt manner, ere three things are farther to be taken notice of. 1. When-er only one or other inconſiderable circumſtance in the ove-mentioned proceſs is changed, either in the burning, king the *lixivium*, evaporating, calcining, or whatever e it be, a different ſalt ſhall immediately and infallibly obtain'd. 2. That in ſuch depurated and cryſtalliz'd ivious ſalts, no ſpecific virtue or property can be diſcovered, ich was in the ſubject before. 3. That all common, and ecially, cryſtalliz'd ſalts of herbs are never pure fix'd kaline ſalts, or ſuch as can be made uſe of in other chemical oceſſes, where a pure fix'd alkaline ſalt is required.

Tachenius and others contend, that freſh herbs yield more t than dried ones; but *Rivinus* on the contrary affirms, at tho' he had obſerv'd it with all poſſible care, and made

M 2 proof

proof with the same herb; yet he could find no differenc
M. *Neuman* thinks that both are in the right in some respe
For, if you treat a vegetable, that contains no subtile,
volatile acid salt, you shall obtain the same quantity of sa
from it, when dry, as when fresh; or if you calcine the fre
herb in the open flame of a fire, and on the contrary, th
dried herb only in a gentle glowing fire, there shall be obtain
almost always the same, or at least a very little different qua
tity of alkaline salt, tho' not of the same quality; and conse
quently, *Rivinus*'s opinion is on this account consonant wi
truth.

But if such a vegetable be treated, as contains either
subtile acid salt, or which is only accidentally mix'd with oth
parts, which very easily evaporate in calcining; then if th
vegetable be fresh, or at least not quite dry, it will yield muc
more salt than another, tho' it may still require one or tw
operations more, and on this account *Tachenius* is in th
right; there are some vegetables, which, notwithstandi
all the care and caution possible in calcining, yet lose the
subtile acid salt, which is necessary to the formation o
alkaline salt; so that but a very small quantity of a fix'
alkali is obtain'd from a very large quantity of these vegeta
bles. But if the exhalation of the acid salt be hindered, i
any respect, then a greater quantity of fix'd alkaline sa
will be obviously produced, which abundantly confirms wh
has been said about the generation and formation of thes
salts. To make this more evident, only take *Guajac* wood
which in deflagration very easily quits its acid salt; if a suf
ficient quantity of the said wood be burnt and calcin'd, an
afterwards the alkaline salt be carefully gather'd from th
ashes, there will be but a very inconsiderable quantity of i
in proportion to that of the wood us'd; the reason of whic
has been just now assign'd, *viz.* because the subtile acid doe
in a great measure evaporate and fly away in the smoke and de
flagration: But if the same quantity of wood be cut into ver
small pieces, or rasped, and boil'd with a large quantity o
water, and then filtred, and you pour fresh water on th
remainder of the wood, and the boiling be continued till th
water be no longer tinged; and if all the decoctions be mix'
together, evaporated and inspissated to an extract, and thi
be slowly dried; and then if both this and the raspings o
little pieces dried apart, be burnt in a gentle glowing fire
and calcin'd, and afterwards made into a lye, and an alkaline
sal

lt be obtained from them in the usual manner; it is surprising ow much greater its quantity is; and this holds as to a great any other vegetables.

Besides the aforesaid fix'd alcaline salts, there is also sold an- her sort from wood, and which is fit for several operations; is, tho' a real alkaline salt, after being entirely made into a e, inspissated and calcin'd, yet is commonly in *Latin*, *High Dutch*, and other languages, only call'd *ashes*, or pot-ash; hich is pretty well known, and by some call'd simply *Sal lcali*; it is commonly prepar'd from the ashes of the harder oods, *viz.* beech, maple, birch, ash, oak, *&c.* From what nd how they are prepar'd, and in what manner the best sort is nown, may be seen in *Kunckelii Arte vitraria*, *Valentini Meusæo Musæorum*, and in a small treatise, entituled, *Ars inctoria experimentalis & fundamentalis.* This pot-ash, as ought and sold, does not constitute an entirely pure alkaline alt, fit for all chemical operations; wherefore, it is to be depu- ated once more, which may be easily done, only pour some old water thereon, but not too great a quantity, let it be dis- olved, filtred, then gently evaporated and inspissated; then it may be made use of in the same manner, and be of equal good- ness with the purest alkaline salt. What remains after decant- ng the solution of the pot-ash, and will not dissolve in cold wa- er, is no longer a pure fix'd alkaline salt, but besides alkaline particles it still participates of an acid, and likewise a vitriolic alt: For, if it be entirely dissolv'd in boiling hot water, then filtred, and the solution, after it has in part evaporated, be laid by for some hours, you shall find white and beautiful crystals, which, in all the trials made on them, resemble a true mean salt, consisting of a fix'd *alkali* and vitriolic acid; and in a word, like vitriolated tartar; what will not dissolve after this second operation with boiling hot water, and remains behind, is entirely useless.

Besides, there is another pure fix'd alkaline salt, which is commonly reckon'd the best, and has hitherto been us'd, above any other, both in chemistry and medicine; and that is salt of tartar; the manner of its preparation is sufficiently known. Whoever would have it more fine and penetrating, may do it two ways, either by calcination, solution, filtration and inspis- sation, or also by calcination, solution *per deliquium*, filtra- tion and inspissation: Both these ways of purifying it, if not several times repeated agree for the most part, and then they only differ, as to the solution *per deliquium*; but if on the one hand,

hand, calcination, and on the other, ſolution *per deliquium* be ſeveral times repeated, then there ſhall be obſerved a very conſiderable difference. 1. Upon repeated calcination, ſome of the ſulphur and acid ſalt that remain, and even ſome of the very ſubtile earthy part, is deſtroyed; nay, the whole compound, that conſtitutes the alkaline ſalt, is perverted, and changed into a pure earth, that no longer diſſolves in water. Were the opinion of the abovementioned authors true, and did the particles of fire concur, as a material ingredient, to the production of alkaline ſalt, it ſhould not deſtroy, but rather augment the quantity of fixed alkaline ſalt, ſince by repeated calcination, there are always freſh particles of fire applied; ſo far are they from being wanting: Nevertheleſs every one may experience the truth of what has been ſaid, as to this repeated calcination. 2. On the other hand, repeated ſolution *per deliquium* and inſpiſſation ſhew the quite contrary; while inſtead of the more ſubtile particles that fly off by calcination, and the more earthy ones that remain behind; here the more crude earthy particles are rather ſeparated and extricated from the fixed alkaline ſalts, and on the contrary, the more ſubtile and fine are retained in theſe ſalts, diſſolved *per deliquium*: Whence ſuch ſalts, by repeated ſolutions *per deliquium*, may at length be ſo far reduced, as not to recover any more a dry conſiſtence, either in cold or heat; but always retain a humid, fluxile conſiſtence, poſſibly like oil of vitriol, and ſuch a ſalt may in the fire rather diſſolve the more tender crucibles, and ſooner penetrate the thicker ones, than appear in a dry form.

It is not uncommon to call fixed nitre, or alcaliſate nitre, a fixed alkaline ſalt; which tho' known to every body, and its preparation reckoned cheap and eaſy; yet M. *Neuman* thinks it not improper to give his thoughts upon this ſubject. The alkaliſation of nitre depends on the following foundation; as ſoon as nitre is mixed and detonated with ſome bituminous or groſs earthy inflammable body, there immediately ariſes partly a reſolution of the nitre, and partly, on the contrary, a generation, or new compoſition of alkaline ſalt; where it is moreover to be noted, that not only a fixed alkaline ſalt, but likewiſe at the ſame time a volatile alkaline or urinous ſalt is generated; if, to wit, nitre be mixed with ſome inflammable earthy ſubſtance, for inſtance, bitumen, pitch, roſin, colophony, tartar, maſtich, common ſulphur, coals, gum ſandarach, &c. then the alkaliſation immediately proceeds, as ſoon as it detonates or deflagrates with theſe; but it is not ſo with the more ſubtile inflammable

com-

mpounds, as highly rectified ſpirits of wine and even diſtilled ls; unleſs peculiar methods be taken with the latter. The eapeſt, and conſequently, the moſt common additament, as is ll known, is pulveriſed coals. But leſt you ſhould imagine, at alkaline ſalt is produced from theſe coals, ſince a whole und of them yields only a few drachms of aſhes, and theſe t only a few grains of fixed alkaline ſalt, it almoſt entirely pends on the nitre itſelf: Whence M. *Neuman* adduces the owing particulars to be farther conſidered. A pound of well purated cryſtalline nitre contains nearly one half of phlegm, fourth of acid ſalt, and another fourth part of an alkaline ditament, which is added, to incorporate the nitrous acid and m the cryſtals of the nitre; whence the whole nitre is a mean t; if with this pound of nitre you mix three or four ounces coals, which of themſelves yield about three or four grains fixed alkaline ſalt, and alkaliſate the ſaid pound of nitre h that quantity of coals, you ſhall obtain 8 or even 10, and the operation be duly gone about, almoſt 12 ounces of good e alkaline ſalt. Now this queſtion ariſes, namely, whence oceeds ſo great a quantity? If there be hardly four ounces of aline additament to the whole pound of nitre. If any one ll farther conſider this matter, and at the ſame time take in at has been ſaid at large on the origin of alkaline ſalt; he y, perhaps, eaſily ſolve it.

M. *Neuman* ſaid in general, that nitre may be alcaliſated th all groſsly earthy inflammable ſubſtances; but he would no means be underſtood, as if in general one and in every pect the ſame fixed alkaline ſalt were thence produced; but theſe inflammable additaments differ among themſelves, ſo is in ſome meaſure different, at one time more or leſs ſubtile, groſs, and at another time highly cauſtic, or even leſs ſo, yet s ſtill fixed nitre, tho' not always and in all reſpects entirely ual, but an *alkali* differing at one time in this, and at anor in that quality, which depends either on the proportion, and that for the moſt part, on the different nature and quaof the additament, as alſo in a peculiar manner on the inmmable principle more and more fixed, or concentred in er fixed matters; whence likewiſe the cauſtic quality primaariſes, for inſtance, if with nitre you mix ſuch a ſubject, in ich there is a large quantity of the inflammable principle, which reaches no farther than the ſurface, nay ſtill diffuſed h a great deal of phlegm, and conſequently, more diſpoſed volatility, as in reſins, pitch, colophony, &c. then, it is true,

there

there is obtained a fixed, or alkalisate nitre, but not near caustic, as if such a subject were added, in which the infla-mable principle is become more fixed and earthy; and this, in-instance, may be easily demonstrated by the additament of mar-tial *regulus* of antimony; in which only the concentred infla-mable principle of an earthy nature from iron is the true ca-of its caustic quality; on which occasion M. *Neuman* owns, th-he could not, so far as he had hitherto considered and observ-the origin of the caustic quality, render any fixed alkaline sa-caustic, without the addition of some mineral substance: S-that this subject requires to be farther considered; since he h-shewn before, in what manner a volatile caustic alkaline sa-may be produced entirely on the same foundation.

Common fixt caustic nitre is prepared, as is well know-from equal parts of martial *regulus* of antimony and of cr-stallised nitre, first pulverised, and then mixed by cementatio-and *regulus* of antimony, quick-lime, and likewise many oth-minerals communicate a strong caustic quality to crude tart-and its salt, nay, even to depurated pot-ash: So that the com-mon caustic, prepared with quick-lime and depurated pot-a-or with salt of tartar, and commonly called caustic *alkali*, sha-dissolve all the tenacious bodies of animals, as feathers, ha-wool, bones, horns, hoofs, *&c.* into a *mucus*, and grease in-short time into a *sapo*; and sulphur into a red liquor. But-this salt be in a liquid form, then it is called *lixivium sapon-riorum*; if it be once more fused in a crucible, and then r-into small oblong pieces, like pieces of a tobacco-pipe, it is ca-led *lapis infernalis alcalicus, cauterium potentiale*, and b-some, *sal chirurgorum*.

At length, besides the two methods of alcalisating nitre, a-ready alledged, and which are more common, there is a thi-sometimes used, namely, if instead of coals, or *regulus*, pulv-rised tartar be added to the nitre: Tho' this preparation be n-ver usually called fixed nitre, but after elixiviation and inspiss-tion, rather the extemporaneous salt of tartar; and if it be n-elixiviated, or depurated, it is called *black flux* among gol-smiths, assayers, miners, coiners, and other artists in meta-But still this fixed nitre is not to be taken so promiscuously f-any other, since according to the different proportion of the tw-ingredients aforesaid, different salts arise. To prepare th-*black flux*, take 1. Either equal parts of both, or 2. One pa-of nitre and two or three parts of tartar: The abovemention-artists are wont to pulverise both a-part, then mix them, an-

afte-

erwards put them into a clean iron mortar, and fire them th a live coal; and generally, without any elixiviation, ration and inſpiſſation, or depuration. 3. If you would ve an entirely white ſalt immediately after the firſt deto- tion, then only take one part of tartar to two or three parts nitre. As to theſe three different proportions; as has en obſerv'd before, the following different circumſtances likewiſe occur. 1. If two parts of nitre be taken to one rt of tartar, then all the nitre is not quite alcaliſated, but ere ſtill remains ſome crude nitre therein. 2. If two or ee parts of tartar be mix'd with one part of nitre, then all nitre, it is true, is reſolv'd and alcaliſated; but on the ntrary, all the oily ſubſtance of the tartar is not conſum'd d burnt, but a large quantity ſtill remains. Whence, erefore, this proportion is peculiarly much more commo- us, proper and uſeful, than any other for fuſing and reduc- metals; and this too (if you will) by a new calcination, ay be freed from the oily particles, and ſo reduced into a rfect alkaline ſalt, which cannot then be called extem- raneous ſalt of tartar. 3. The mean proportion, where ual parts are taken, ſeems to be the beſt; however, it nnot be denied, but it ſtill retains ſome crude nitre.

In the laſt mentioned proportion, and much more in that two parts of nitre, during the deflagration, there ariſes a l ſeparation of the particles or conſtituent parts of tartar; d the four following particular circumſtances occur. 1. The eateſt part of the oily ſubſtance of tartar is not only reſolv'd, t conſum'd in ſuch a manner, that inſtead of that large antity of oil of tartar ſeparated in diſtillation, and which pears in a liquid form; here in detonation with nitre, ere is no other ſign of it obſervable, than a little dry ſoot. Another remarkable circumſtance in the formation of this temporaneous ſalt of tartar, is this, namely, that whereas, ring the diſtillation or burning of the tartar, on the one nd, there is a ſeparation of the oil, the ſpirit and phlegm; d on the other, a new formation of fix'd alkaline ſalt, conſe- ently, the whole proceeds ſlowly, and requires ſome time; t here the proceſs happens at one and the ſame time, and in e ſingle detonation. 3. It is well known, that tartar and nitre ither of them a-part) if diſtill'd, yield an acid ſpirit; but re, where theſe two ſaline compounds deflagrate together, ere is none at all, but quite a different, nay, a contrary, d a new form'd, urinous ſubſtance preſents itſelf to view.

4. Since in the common method of burning tartar by itſel
without any additament, there always remains ſome ſma
portion of an indiſſoluble earth, after the elixiviation of th
alkaline ſalt; here in the preparation of extemporaneou
ſalt of tartar, there is found almoſt no indiſſoluble earth a
all, but the whole is in a moment, and with an incomprehe
ſible quickneſs changed into a pure ſalt.

A Lunar Eclipſe October 10. 1725. *at* Briſtol; *b*
Mr. Burroughs. Phil. Tranſ. N° 392. p. 37.

THE cloudy weather at *Briſtol* prevented us from ob
ſerving the beginning of the eclipſe, and of the tota
darkneſs; but Mr. *Burroughs* obſerv'd pretty exactly th
firſt appearance of light, after the total darkneſs, and th
end of the eclipſe; and their reſpective times are, as follow

	h.	′	″	
Beginning of light	7	31	20	apparent tim
End of the eclipſe	8	29	30	

Some ſmall time before the renewal of the true light, ther
appeared a remarkable brightneſs upon the moon's eaſter
limb, that was likewiſe diffus'd about her limb, to a ſenſibl
diſtance from the moon. If others, who are more ſkill'd i
theſe matters, have made the like obſervation, Mr. *Burrough*
ſhall no longer doubt of the moon's having an atmoſphere.

Part of two Human Skeletons *petrified*; *by Dr.* Joh
James Scheuchzer. Phil. Tranſ. N° 392. p. 38. *Tranſ*
lated from the Latin.

THERE are but few parts of the human body remain
ing ſince the deluge. Dr. *Scheuchzer* had hitherto
amongſt a pretty large collection, no more than two petrifie
vertebræ of the back, of a ſhining black colour; and at tha
time he had preſented to him a very remarkable relique
inclos'd in an *Oeningen* flaky ſtone, in which might very diſ
tinctly be obſerved a great many parts of a human head, a
the circumference of the ſkull, the *os frontis*, *oſſa ſincipiti*
and *occipitis*, the orbit of the eye, pieces of the baſis of th
brain and of the *medulla oblongata*, the interior prominence o
the *os occipitis*, that divides the lobes of the *cerebellum*, th

7 *vertebr*

vertebræ of the neck, partly bare and partly cover'd with a etrified crust; and this is the orthographical section, as it ere, of the hinder part of the head.

Afterwards the Dr. procur'd another relique from the foresaid stone quarry, that was larger than the former, older d more curious. There were inclos'd in the said flaky stone om the skeleton of an adult person and his anterior part, e periphery of the *os frontis*, the *os jugale*, the orbits of e eyes, the tables of the skull, together with the *diploe*, e vestiges of the *foramen infraorbitale* for the passage of the erves of the fifth pair, parts of the brain itself or *dura mater*, e *ossa cribrosa* and *spongiosa*, the *os vomeris* that divides the ose, a portion of the fourth maxillary bone, which constitutes e cheeks, parts of the nose, a portion of the *masseter*, an rthographical section passing thro' the *apophysis condyloides* f the lower jaw, as far as the angle of the said jaw, 16 continued *vertebræ*, most of them having the transverse processes, e extremity of the right clavicle, join'd to the *scapula*, the iddle of the left cover'd with a stony crust. From the proportion of this skeleton, the Dr. supposes, that the whole ature of the man was the same, with his own, *viz.* 58 *Paris* ches and a half.

A Contrivance to avoid the Irregularities in the Motion of a Clock, *occasioned by the Action of Heat and Cold upon the Rod of the* Pendulum; *by Mr.* George Graham. Phil. Transf. N° 392. p. 40.

WHEREAS several, who have been curious in measuring time, have taken notice, that the vibrations of a endulum are slower in summer than in winter, and very ustly suppos'd, that this alteration has been owing to the hange of length in the pendulum itself, by the action of heat nd cold upon it, in the different seasons of the year. With view, therefore, of correcting, in some measure, this defect f the pendulum, Mr. *Graham* made several trials, about the ear 1715, in order to discover, whether there were any considerable difference of expansion between brass, steel, iron, opper, silver, &c. when expos'd to the same degrees of heat, s near as he could determine; imagining it would not be ery difficult, by making use of two sorts of metal, differing onsiderably in their degrees of expansion and contraction, o remedy, in a great measure, the irregularities to which ommon pendulums are subject. But tho' it be easily disco-

verable, that all those metals suffer a sensible alteration of their dimensions by heat and cold; yet he found their differences from each other as to quantity, were so small, as gave him no hopes of succeeding this way, and made him leave off prosecuting this matter at that time. In the beginning of *December* 1721, having occasion for an exact level, besides other materials he made trial of, quicksilver was one; which, tho' he found it nowise proper for a level, yet the extraordinary degree of expansion he observ'd in it, when placed near the fire, beyond what he imagined to be in so dense a fluid, immediately suggested to him the use that might be made of it, by applying it to a pendulum. In a few days after, he made the experiment, but with much too long a column of mercury, the clock going slower with an increase of cold, contrary to the common pendulum; however, it was a greater confirmation of the advantage to be expected from it; since it was easy to shorten the column in any degree required. The only doubt he entertained was, lest there should not be a proportionable expansion and contraction between the mercury and the rod of the pendulum, thro' the various degrees of heat and cold, from the one extreme to the other. In order to make this experiment the more convincing, he placed the clock in a part of the house the most expos'd of any to the changes of heat and cold, the room having no fire in it in winter, and expos'd to a south sun, with leads above it, which made it exceeding hot in summer. He hung a thermometer by it, and he had likewise another clock, at no greater distance from it than was necessary to keep the cases from touching each other. This clock had been made some years before, with extraordinary care, having a pendulum about 61 pounds in weight, and vibrating not above one degree and a half from the perpendicular; and which, in a more temperate situation, had not alter'd above 12 or 14″ in 24 hours between winter and summer; but in this place it alter'd 30″ a day, between the hottest and coldest weather in 1722, a year no way remarkable for either extreme. But this great alteration was owing to the abovementioned situation, and which he made choice of for the sake of making the experiment the more sensible. The two clocks being firmly screwed to a party-wall, he began to make the first trial of this kind of pendulum *Dec.* 18, 1721: and by *Jan.* 3, perceiving the column of mercury considerably too long, he procured a shorter glass, which he got ready by the 8. and made use of till the beginning of

Fig. I.

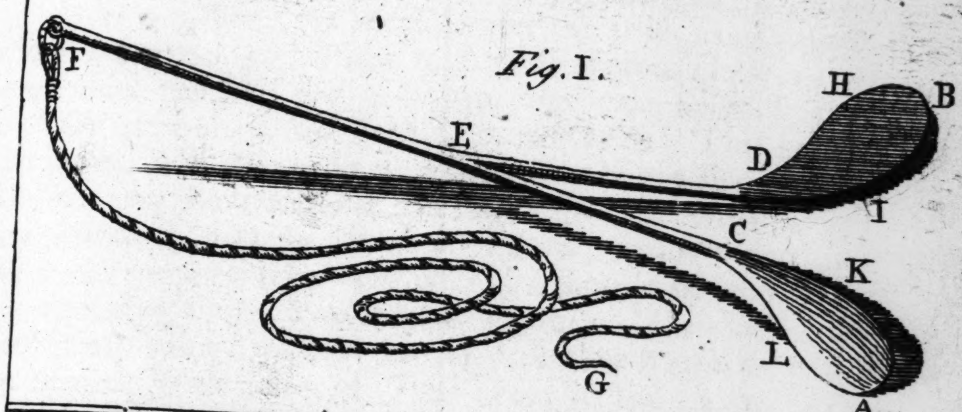

Fig. II.

G. Smith del. *Hulett Sculp.*

f *June* following; by which time he was well ſatisfied of the dvantage of the contrivance, notwithſtanding both theſe pendu- um's were but rudely executed, and this laſt had the column f quickſilver too ſhort, but much nearer the true length than he firſt. This encouraged him to provide another glaſs, a lit- le longer than the laſt, and to beſtow more care upon all the arts of the pendulum, that required exactneſs. This being iniſhed by the 9. of *June*, he then began to obſerve the motion f the clock, by the tranſits of the fixed ſtars, as often as the eather permitted, making uſe of a teleſcope, which moved in he plane of the meridian; with this inſtrument he could be ure of not erring above two ſeconds in time. The clock was ept conſtantly going, without having either the hands or pen- ulum altered, from the 9. of *June* 1722, to the 14. of *Oct.* 725, being 3 years and 4 months.

For the firſt year, Mr. *Graham* wrote down, every day, the ifference between the two clocks, with the height of the ther- mometer, not omitting the tranſits of the ſtars, as often as the eather was clear. The reſult of all the obſervations was this, hat the irregularity of the clock, with the quickſilver pendu- um, compared with the tranſits of the ſtars, did not exceed, hen greateſt, a ſixth part of that of the other clock, with the ommon pendulum; but for the greateſt part of the year, not bove an eighth or ninth part; and even this quantity would ave been leſs, had the column of mercury been a little ſhorter; or, it differed a little the contrary way from the other clock, oing faſter with heat, and ſlower with cold; but he made no lteration in length, to avoid an interruption of the obſervations. n order to confirm this experiment the more, about the begin- ing of *July* 1723, he took off the heavy pendulum from the ther clock, and made another with quickſilver; but with this ifference, that inſtead of a glaſs-tube, he made uſe of braſs, nd varniſhed the inſide, to ſecure it from being injured by the ercury. This pendulum Mr. *Graham* made uſe of ever after, nd found it about the ſame degree of exactneſs with the other. he reaſon, why this kind of pendulum is more exact than the ommon ſort, will be evident to any one who conſiders, that as eat lengthens the rod of the pendulum, it at the ſame time in- reaſes the length of the column of mercury, and its center of gra- ity is moved upwards: And when the rod of the pendulum is hortened by cold, the column of mercury is likewiſe ſhortened, nd its center of gravity carried downwards. By this means, f the column of mercury be of a proper length, the diſtance between

between the point of suspension and the center of oscillation of the pendulum, will be always nearly the same, upon which the exact motion of a clock principally depends. Were the pendulum of a clock to remain invariably of the same length; yet some little irregularities would appear in its motion, from the difference of friction, arising from the imperfections of the materials, as well as different degrees of foulness; upon which account, the force, communicated to the pendulum, would not be constantly equal, which would cause some little alteration: But when the pendulum is very heavy, and vibrates in a small arch, and the workmanship of all the parts is well executed, there will be very little inequality in the motion, besides what is owing to heat and cold.

In making use of quicksilver for a pendulum, by varying the diameter of the vessel that contains it, or the thickness of the rod of the pendulum, whether it be of brass or steel, they may be reduced nearly to an equality, as to the receiving, or retaining the impressions of heat or cold, upon which the greater regularity of the motion depends; and particular care should be used to free the mercury from all blebs of air, otherways their great and sudden expansion, or contraction, may occasion a considerable disorder; but the air may as easily be excluded in this way, as in a barometer; and the considerable specific gravity of quicksilver renders it a proper material for the weight of a pendulum.

The Sequel of the Account of Alkaline Salts *in* Phil. Trans. N° 392; *by M.* Neuman. Phil. Trans. N° 393. p. 45. *Translated from the* Latin.

AS to the lixivious salts of plants, it is first to be noted in general, that if they happen to differ in some one respect or other, this difference is not owing to any specific quality and peculiar virtue in this or that plant, before it is burnt and calcined, but primarily depends on two other circumstances (as that most expert chemist, M. *Stahl*, has observed) namely, 1. On the different structure either of the finer or grosser parts of the plant, and 2. On the operation, or different methods of treating it.

1. On account of the structure, a quite different alkaline salt is obtained, for instance, from the fine and tender leaves, than from the stalks of the same herb; from the *petala* of flowers, or from the whole flower, than from the wood itself; from the seeds than from the roots; from the expressed juice, than from the

he extract, prepared from the dry plant by means of a large uantity of water and strong boiling; and from tender fresh erbs, than from such as are very dry; and so on.

2. On account of the operation, a quite different alkaline alt is obtained from a gentle smoaking burning, and incinera-ion, than from a strong flaming one; from an elixiviation with old water, than with warm, nay boiling hot water; with a mall quantity of water, than with a larger; by a single gentle alcination than by a strong and repeated one; by again dis-olving the inspissated salt in a large quantity of water, than *per deliquium*; and that again according to several other circum-tances. But, unless lixivious salts happen on purpose, as by re-eated calcinations or solutions *per deliquium*, to be reduced to o remarkable a difference, any other difference from the com-mon operations is of little or no importance, except some mine-al body happen to be put to it, either designedly, or thro' carelessness or ignorance.

Should therefore any difference be discovered in the common ure lixivious salts of herbs, it would consist only in this, amely, that the one is a more pure alkaline salt than the other; he one more subtile, and the other more gross; the one more arthy than the other; the one a little more caustic than the other; the one impregnated with more particles of an oily in-gredient, or even a greater quantity of acid spirit, and conse-quently, more crystalline than other salts. However, they all are lixivious, fix'd and alkaline salts; but not all pure; since the purest of them are still impure, and unfit for other chemical uses; whilst most of them still manifestly retain somewhat of a superfluous acid; and some again a superfluous earth and other accidental adventitious admixtures, if not voluntary, and idle additions, as imaginary contrivances for producing beautiful crystals; so that if we consider the crystallised lixivious salts of vegetables, commonly prepared in the shops, we cannot help wondering, why such salts are still made use of in the modern practice of physic, since the physician does not know, what salt he prescribes his patients; for, such crystallised salts are neither pure alkaline nor entirely saturated neutral salts; and conse-quently, no one can exactly tell, how much *alkali* and acid one drachm or one ounce of such a beautiful crystallised salt may contain; or whether the acid, contained therein, and that dis-poses it to shoot into crystals, be a vegetable or mineral acid.

Were M. *Neuman* to prescribe in his practice a fix'd alkaline salt, he would take pure salt of tartar or even depurated pot-

ash;

ash; and if an acid salt were proper, he would chuse the fittest of the manifest pure acids; and were he to make use of a mean salt, he would prescribe such a one of the common, highly saturated mean salts, as would best suit the present case; for, then he would be sure of what he had from the apothecary's, and what his patients took; and he would omit using the other uncertain, and imperfectly saturated salts, that are neither pure acid, alkaline, nor mean or neutral salts.

M. *Neuman* would give the following advice, *viz.* that if any one would expect any one or more particular effects from this or that vegetable lixivious salt; and if it were possible for any one fixed alkaline salt to have some peculiar virtue in physic above any other, to let this herb burn as gently as possible, and not calcine it violently, much less burn it strongly; and least of all to calcine the ashes or salt; then probably, the peculiar virtue expected from it, would in some measure be preserved in the few remaining oily particles, in which alone, it must necessarily be lodged; since, on the contrary, he is assured, that (if it be burnt in a flaming fire, calcined, and depurated, without due care) it can really yield nothing specific or singular.

Tho', therefore, M. *Neuman* could, for experiment sake, from one and the same plant (without any adventitious additament, but only by means of fire and water) prepare and produce, not to say 40 or 50, but several kinds of fixed alkaline salts, one of which should always differ from the other, in some measure, tho' but little in any one particular circumstance, as sometimes in the external appearance, and sometimes in some trivial internal circumstance: Yet on the other hand he knows very well, that not only these various kinds of salts, produced by art from one and the same plant, but that all lixivious salts of herbs; nay, farther, that all fix'd alkaline salts, may be reduced to the same appearance, virtue and efficacy: So that in this respect there is but one single kind of fix'd alkaline salt in all the vegetable kingdom; since either this one salt may be reduced to various forms and made to produce various effects, or even the whole species to the same appearance and effect: So that if we had lixivious salts, prepared from 100 herbs and upwards before us, we could not determine any salt, much less any subject, from which such an alkaline salt was produced: For, no one can so much as know in general, whether it be produced from a wholesome, or from a highly poisonous, or noxious vegetable; whether the subject be sweet or strong scented; or whether it be entirely destitute of any smell or stench; whether it be savoury or insipid;

purging

urging or aſtringent; antiſcorbutic, epileptic, or antifebrile; metic, or diuretic; narcotic or ſternutatory; cauſtic or balſa- nic; whether it be prepared from the wood or root; bark or ith; flower, or ſeed, or juice; or whatever elſe it be, from vhich ſuch an alkaline ſalt is prepared.

This M. *Neuman* thinks may ſerve to convince ſuch as are ſtill f opinion, that fixed alkaline ſalts retain the ſpecific virtues of e herbs, and other vegetables, from which they are produced; that they take the ſalt of cloves and cinnamon to be a ſtoma- hic, that of henbane and poppy, narcotic, that of jallap-root purger, that of tormentilla root, or china china bark, aſtrin- ent or antifebrile, that of *ipecacuahna* and hedge-hyſſop eme- c, that of ſpurge to be cauſtic, and that of hemlock to be oiſonous and deadly: So that the reſuſcitation and revivification f vegetables cannot be ſo eaſily conceived, ſince it may be de- onſtrated, that all ſuch lixivious ſalts in the whole vegetable ingdom, may be reduced to the ſame figure and virtue, nay, nd thoſe from the ſame herb (as has been ſaid) to ſo great a va- ety of ſalts: Whence M. *Neuman* owns, that it is not poſſible om the aſhes or *lixivium* to reproduce even the external figure f that vegetable, from which thoſe ſalts are prepared, or re- reſent it in the ſalt itſelf from ſuch pure ſalts; he ſays, ſuch ure ſalts; ſince he himſelf had ſeen more than once curious gures of herbs, made of fine braſs wire and thread, and either ut into or hung over the concentred *lixivia*, and around theſe, ryſtals were formed; which were impoſed upon the ignorant, real repreſentations of the reſuſcitation of plants: In ſhort, l the pretended reſuſcitations by human art, have no real foun- ation; but are only mere chimeras, and to be ſeen no where ut in books.

There is likewiſe ſome difference found, in one reſpect or ther, even between the purer fixed alkaline ſalts themſelves, hich are known to be more adapted to chemical uſes; all hich a ſkillful chemiſt may remove, and render them entirely qual, and reduce them to the ſame figure and virtue.

Dr. *Hoffman* has a whole chapter to this purpoſe in his ſecond ook of *Obſ. Phyſ. Chym. ſelectiorum* N° 29. where he only ys down the differences, which conſiſt in the following par- culars.

1. If oil of vitriol be poured on fixed nitre, then during the fferveſcence the true ſpirit of nitre, or *aqua fortis* exhales. But this never happens, if oil of vitriol be poured on ſalt tartar or on pot-aſh. 3. But again there is ſome difference

observed in these two last; since from salt of tartar (upon pouring oil of vitriol) the surface acquires a blackish pellicle, the liquor itself becomes blackish, and fixed nitre yields a quite different scent, from that of nitrous spirit. 4. But all this is not observed from pot-ash, since it neither affords spirit of nitre, a pellicle, or a blackish tincture or solution, but a peculiar vapour. 5. These nitrous fumes do also become manifest, if oil of vitriol be poured on extemporaneous salt of tartar, prepared from two parts of nitre and one part of tartar. 6. And this again does not happen in such extemporaneous salt of tartar, prepared with a little addition of nitre. 7. Clear and pellucid glass cannot be prepared from salt of tartar and sand. 8. But very well from extemporaneous salt of tartar, as also from pot-ash. 9. From salt of tartar, or pot-ash, fused in a crucible, and adding coals, there results a species of *hepar sulphuris*, which neither constitutes fixed or caustic nitre. 10. Salt of tartar and caustic nitre yield a tincture with highly rectified spirit of wine, which yet neither pot-ash, fixed nitre, or that extemporaneous salt of tartar, prepared from two parts of nitre and one part of tartar, afford.

It is true, Dr. *Hoffman* has observed these differences, but not assigned the reason, whence they happen, and wherein they consist, whether, and how these fixed alkaline salts, may be meliorated and corrected; much less how these differences may be removed, and these salts rendered entirely equal; since one and all of them proceed in a natural order, the difference may be very easily perceived and corrected and one salt be made equal to another: So that, 1. From all that is abovesaid, not so much as one single phenomenon, is peculiar to one or other salt; and 2. No other difference should be observed between all pure fixed alkaline salts, than that of their different degrees of causticity.

1. Oil of vitriol, poured on any of the said salts, should not produce a peculiar, but always the same vapour and scent. 2. All these salts should equally be reduced into clear, pellucid glass. 3. Each of them should likewise tinge highly rectified spirits of wine of a beautiful colour; and so on.

Thus it is not at all surprising, that oil of vitriol should discharge a nitrous spirit out of fixed nitre, or extemporaneous salt of tartar, prepared with two parts of nitre; while, if this happen, some portion of crude nitre must still necessarily remain in such an alkaline salt; whence the fumes arising from it (which are no other than what constitute the true spirit of nitre) are as natural

s if, according to *Glauber*'s method, oil of vitriol were poured n crude nitre; when on the contrary, even no ſpirit of nitre an be freed, nor nitrous fume expected from common ſalt of tarar, pot-aſh, and other alkaline ſalts of this kind, to which no itre has been added; nay to which (obſerve this) it has not een ſuperfluouſly added.

That this may the more evidently appear, only take the ſame ixed nitre, or the ſame extemporaneous ſalt of tartar, from hich, upon pouring oil of vitriol thereon, a nitrous fume apears, and let it be mixed anew with ſome one or other inflamable ſubject of the vegetable kingdom; for inſtance, let the x'd nitre be mix'd with a larger quantity of coals, and exmporaneous ſalt of tartar with a larger quantity of tartar, and t it deflagrate as before; or for the greater certainty, let the roportion of coals, or tartar, be increaſed ſtill more, that the efore ſuperfluous, and ſtill crude nitre may find more inflamable earth, with which it may kindle and be alkaliſated, hat, conſequently no more crude nitre may remain; then it is ue, from this recollected ſalt, upon pouring oil of vitriol, no ore nitrous ſpirit ſhall fume but rather quite a different apour.

That oil of vitriol with ſalt of tartar cauſes a quite different ent from that of ſpirit of nitre, as alſo a blackiſh pellicle and ark ſolution, is owing to nothing elſe but the ſuperfluous empyeumatic particles that ſtill remain in the ſalt of tartar. But if lt of tartar be freed in a proper manner from theſe particles; hen there ſhall no longer be obſerved any ſtrong ſcent, or lackiſh ſubſtance, which oil of vitriol contracts with all oily odies: Hence no ſuch blackneſs ever appears, or can be deonſtrated from pot-aſh, becauſe, by means of calcination, they ave been more freed from empyreumatic oil; nor from pure lt of tartar, if ſufficiently calcined; but upon pouring oil of itriol all continues clear and white.

In like manner it is owing only to this empyreumatic oil in alt of tartar, why with ſand it does not produce clear pellucid laſs; but as ſoon as it is freed from this ſuperfluous oily ſubtance, then it is as proper for making pellucid glaſs, as pot-aſh, r even extemporaneous ſalt of tartar. This is evidently conirmed by this, namely, that while it is preparing, during the etonation with nitre, all the oily ſubſtance of the tartar is urnt and deſtroyed: Hence this alkaline ſalt, tho' it have tarar in its preparation, does conſequently make clear glaſs, in

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the ſame manner, as pot-aſh, to which no tartar is made uſe of.

Likewiſe this inflammable principle, that is ſtill preſent in tartar, or that is imparted to the alkaline ſalt in ſome other way, as what alone (as has been often taken notice of) contributes to its cauſtic quality, is that ſubſtantial matter which gives a tincture to highly rectified inflammable ſpirits; tho' the burning ſpirit, as a *menſtruum*, if it abound with oil, may likewiſe in part contribute a good deal to it, and the tincture be accelerated and increaſed ſo much the more. It, therefore, is not ſurpriſing, if you obtain no tincture from extemporaneous ſalt of tartar, prepared with two or three parts of nitre; ſince all the inflammable ſubſtance, which is abſolutely neceſſary to the production of the tincture, is deſtroyed by it.

Tho' M^r. *Neuman*, in defining theſe ſalts, ſaid, that they conſiſt of an earth, in which by means of the fire, as by roaſting, there is wedged ſomewhat of the more fixed and at length more concentred acid ſalt and ſulphur; yet he ſaid at the ſame time, as appears from the words of the definition, *a little or ſome*, that either the ſuperfluous acid or ſulphur; nay, that even the ſuperfluous earthy parts themſelves were not only not neceſſary, but in reſpect of purity and perfection, requiſite in ſuch ſalts, were hurtful and noxious.

And if ſome pure fixed alkaline ſalt be again treated and mixed, either on purpoſe, or by accident, with a pretty large quantity of one of the aforeſaid conſtituent parts whether it be too much of an acid, ſulphur, or earthy parts, or with two of theſe together; then this new compound becomes not only of a quite different denomination, but acquires quite different qualities and virtues, from what alkaline ſalt alone had before.

For, if a fixed alkaline ſalt be mixed with an acid ſalt, till, after the remitting of its effervеſcence, and ſeparation of the water (which only ſerved as the vehicle of ſolution) it ſhall not, upon mixing ſome of it with the ſyrup of violets, alter its tincture, either to a red or green; then this new mixture is called a mean ſalt, a *ſal ſalſum*, or neutral ſalt: And if a mineral acid were made uſe of, then this new compound eaſily ſhoots into cryſtals; but if a vegetable acid, then it ſhoots into cryſtals with difficulty, or not at all.

Since very different acid ſalts occur in chemiſtry; according therefore, to the nature of theſe acids (if one or other of them be mixed with a fixed alkaline ſalt to ſaturation) the mean ſalt thence produced commonly takes its denomination; for inſtance.

1. If

1. If a fix'd alkaline ſalt be mix'd with a vitriolic or ſulphu-eous, acid in ſuch a manner, as to ſhoot into cryſtals; then the ryſtalline mean ſalt, ariſing from both theſe, is commonly all'd vitriolated tartar or *ſal polychreſtum*. 2. If a fix'd alka-ine ſalt be mixed with the acid of nitre and cryſtalliz'd, there hence ariſes a cryſtalline nitre, which is commonly call'd *itrum regeneratum* or *tartarus nitratus*. 3. If ſaturated with he acid of common ſalt, there thence ariſes a common *ſal re-eneratum*, call'd *ſal embryonatum*, or *digeſtivum Sylvii*. . With the acid of vinegar there is produced *terra foliata artari*, or *arcanum tartari*. 5. With the acid of tartar here ariſes a ſoluble tartar, or *tartarus turtariſatus*. 6. With itron juice *tartarus citratus*. 7. The alkaline portion that eſides in common ſalt, produces with a vitriolic acid a mean alt, call'd *ſal mirabile Glauberi*. 8. And if the alkaline ortion of nitre be mix'd with the ſaid vitriolic acid; ſo that a ryſtalline mean ſalt be obtained from both theſe ingredients, ho' it no ways differ as to its fouudation, from vitriolated tartar, et is call'd by another name, which it has had for ſeveral ears, namely *arcanum duplicatum*, or *panacea Holſatica*. . But if only ſome ſuperfluous acid be introduced either from he vegetable itſelf, or any other way into the common alkaline alts of plants, then there commonly ariſes thence the cryſtalline ixivious ſalts of plants.

If a fix'd alkaline ſalt be mix'd with ſome inflammable ſub-ſtance, either fat, or expreſs'd oils, as alſo thoſe diſtill'd, after ertain methods of operation, thence at firſt is produced (as is well known) a ſapo, and then a volatile ſalt and urinous ſalt, but if any animal empyreumatic oil be added, then this alka-line ſalt, only impregnated therewith, will in ſome curious operations conſiderably diſtinguiſh itſelf beyond vegetable ſul-phur: If bituminous and vitriolic acid compounds (as in common ſulphur) be mix'd with a fix'd alkaline ſalt, thence is produced a *hepar ſulphuris*. In fine, if a pure fix'd alkaline ſalt be ſaturated in the fire with a pellucid earth, as common ſand, flint, pyrites, grit, &c. then not only a ſubtile acid ſpirit ſhall be obtain'd from thence, but the whole compound change to a pellucid and tranſparent earth, ſtill fuſible in fire, tho' an indiſſoluble earth in water, namely into glaſs, or even with other additaments, into porcellian.

In general, fix'd alkaline ſalts do not eaſily unite together, or theſe ſalts do not eaſily diſſolve any thing, that does not contain either ſome inflammable, or acid quality; and one more

more readily and intimately than the other. Thus, 1. Fix'd alkaline salts easily dissolve common sulphur, because an inflammable acid compound; but they are likewise easily separated and freed again from it. 2. They likewise easily dissolve, and unite, with grease, fat and express'd oils, and they communicate to these fatty substances a disposition of readily dissolving with them in water, and of incorporating with water; when otherwise without a fix'd alkaline salt, they will not incorporate at all. 3. Fix'd salts dissolve and at length likewise unite with essential distill'd oils, tho' with some little difficulty.

And with the same difference that the above-mentioned two kinds of oils do more readily or more difficultly unite with an *alkali*, does their separation in like manner succeed; while these fix'd salts may be very easily separated from grease, fat, and express'd oils; and on the contrary, very difficultly, or not at all from ethereal and essential oils; especially, if once intimately united with them, and both have been reduced into a perfect sapo. 4. A separation from glass, it is true, succeeds somewhat slowly; yet it is possible, tho' in one sort sooner than in another (most easily of all in flint glass) provided they be again mix'd according to art with an *alkali*, and treated in a proper manner. 5. This union and solution of fix'd alkaline salt with acid ones, does the most easily and readily suceeed; and that in a three-fold manner. 1. Either by commixture, if both be simply join'd together withont the intervention of any thing else; or 2, by precipitation, if either 1, an acid salt, or 2, a fix'd alkaline salt were before mix'd with any other body; or 3, even without precipitation, while the acid salt has been mix'd before with some other; but this dissolv'd in an acid is not separated therefrom by precipitation.

1. This union is produced by commixture, if a fix'd alkaline salt, dissolv'd in water, or even *per deliquium* be mix'd with a spirit, or some acid liquor, as may be observ'd in the qrocess for obtaining the above-mentioned several mean salts, for instance, in the preparation of the *terra folliata tartari*, of *tartarus tartarisatus*, regenerated nitre, vitriolated tartar from spirit of vitriol, and oil of tartar *per deliquium*, &c.

2. This union is effected by precipitation, 1, if an acid salt, have been previously dissolved and combined with some metallic, or even animal acid, which may happen several ways, either designedly or accidentally, and is frequently the case in pharmaceutical chemistry; for instance, in the preparation of

aurum

urum fulminans, where, if a ſolution of alkaline ſalt be oured into *aqua regia*, impregnated with a ſolution of gold, he union of the alkaline ſalt with both the acids, namely, the itre and ſalt, happens, by the precipitation or proſtration of he gold, This, moreover, is manifeſt in the preparation of hite precipitate, in that of *tartarus vitriolatus Tachenii*, nd ſeveral other metallic precipitations. In animal ſolutions may be obſerv'd the ſame union of an alkaline ſalt with an acid, aus'd in like manner by precipitation; for inſtance, in the reparation of mageſterie of hartſhorn, and other parts of nimals. On the other hand this union is effected by way of recipitation; as in the preparation of *ſulphur auratum antimonii*, where the alkaline ſalt is found impregnated with the iſſolv'd parts of ſulphur, which unites with acid ſalt, put to it, ut quits by precipitation the ſulphur, before diſſolv'd: This, moreover, appears, if common ſoap, diſſolv'd in water, be recipitated with an acid; and this always happens, where groſsly fatty reſinous, or ſulphureous ſubſtances have been previouſly diſſolv'd and combined with a fix'd alkaline ſalt, but afterwards precipitated with an acid.

The third method of union, which is effected without precipitation, yet not ſimply by commixture, while there is ſtill ſome intermediate ſubſtance, may be done two ways; either, 1, if an acid ſalt were before united with an urinous volatile ſalt, and reduced into ſal-armoniac, and, if to this mean volatile ſalt a fix'd alkaline ſalt be added, the acid ſalt, eſpecially, if only a gentle heat be applied, ſuddenly combines with this adventitious fix'd *alkali*, and quits the volatile alkaline ſalt, it before contained, and which then, as being left alone, and a very volatile body, may be eaſily expelled by the ſaid gentle heat; or 2, if the alkaline ſalt were before united, with even a weaker acid, and a ſtronger acid pour'd on it, and this new compound expoſed in a retort to the fire, then the weaker acid is abſolutely obliged to yield to the ſtronger, and ſeparate by diſtillation, which manifeſtly appears in the diſtillation of ſpirit of nitre and *ſal fumans*, &c.

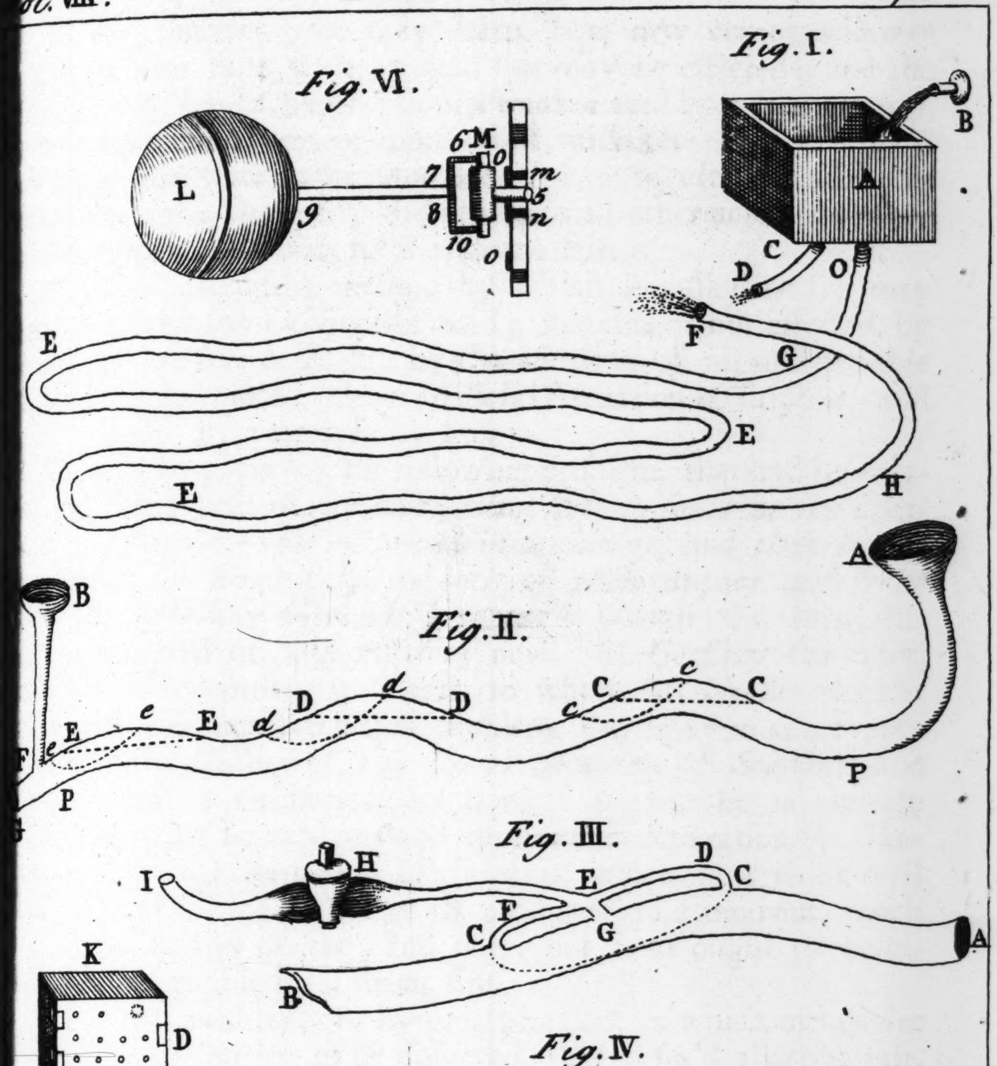
But tho' this union of a fix'd alkaline ſalt with all acid ſalts, be very amicably perform'd; yet it is not thence to be imagined, that this happens equally in all, or that it unites with one, in the ſame intimate manner, as with another: By no means; for here there is a conſiderable difference; ſince a cloſer union or affinity, may be obſerved between fix'd alkaline ſalts and this or that acid ſalt: And ſince the different degrees of this affinity are

are of considerable consequence in chemistry, and the knowledge of them very necessary to a practical chemist, M. *Neuman* cannot avoid saying something on this subject; especially, since from the combination of a fix'd alkaline salt with acid salts, and from the different degrees thereof, we may best judge of the strength of acids, namely, in so far as it relates to the combination with a fix'd *alkali*.

A pure fix'd alkaline salt does best of all, and most intimately unite with the strongest primogeneous pure acid, and a more fix'd vitriolic and sulphureous acid: But if this acid be still mix'd with an inflammable principle, as common sulphur, then it is not intimately united with an *alkali*, but there is only a superficial solution or cohesion; and as has been observed above, there is produced a bare *hepar sulphuris*, from which the *alkali* may again be separated very easily, nay with the weakest vegetable acid.

This firmer sulphureous acid does in a great measure adhere to alkaline salt, so as not to be freed from it, at least by the weakest acid, if it have lost by deflagration, only a part of the sulphur, with which it was united; which is pretty evident, if during the deflagration of the sulphur, the fix'd alkaline salt be applied in such a manner, as that the volatile spirit of the sulphur be entangled therein: However, this adhesion is not sufficiently firm; since this volatile spirit may again be expelled from thence and freed from the fix'd alkaline salt, by means of a more fix'd and concentred vitriolic acid of a peculiar nature, that is yet more pure, as entirely depurated from the sulphur; on no other account, than that it still participates in a good measure of the inflammable principle; whence that acid is still volatile and subtile, and consequently not so pure and fix'd as rectified oil of vitriol: But if such a volatile sulphureous spirit be expos'd to the open air only for some time, then this subtile sulphur entirely exhales; while on the contrary there remains a pure fix'd acid, which no ways differs from, and in all proofs, is found equal to any other acid, distill'd from vitriol or alum; and which will then very firmly unite with a fix'd alkaline salt in the same manner, as any other pure, fix'd vitriolic acid. Next to this sulphureous or vitriolic acid, this fix'd alkaline salt very readily unites with the acid, or spirit of nitre; and then with the acid of common or sea salt; and lastly, the weakest of all with vegetable acids, such as that of vinegar, tartar, spirits of wood, and sugar, and with lemon-juice, *Rhenish* wine, &c.

Hence



G. Smith del. Hulett Sculp

Hence, therefore, we may learn, how new combinations of fix'd alkaline salts with an acid salt may be effected; and the same *alkali* be discharged from a weaker acid by a stronger one, but in this graduation we must avoid mistakes: For, if a fix'd alkaline salt were to be united with a pure vitriolic acid; as this then is the strongest, and separates all other acids, no other could deprive it of this fix'd alkaline salt.

M. *Stahl*, shewed a method by which an alkaline salt may again be freed from a vitriolic acid; but this is not effected by means of another acid, but by the addition of an inflammable principle; in a word, by an artificial formation of sulphur, and consequently, by a process by fire.

In 1718 he proposed the following problem, that had no relation to this genesis of sulphur; *viz. how to separate the alkaline salt from the vitriolic acid in a moment, and that in the hollow of the hand only, as both of them appear under the form of crystalline vitriolated tartar?* Which is a thing, hitherto unheard of, and entirely new. M. *Geoffroy* the elder, professor of chemistry at *Paris*, to whom M. *Neuman* had at that time communicated this problem, did in 1720 cause publish it in the *History of the Royal Academy of Sciences*, and he subjoined a chimerical solution of it; but he is entirely mistaken, since he explains and applies this separation of alkaline salt, to the formation of sulphur, which is a thing well known, and which is not at all produced in a moment, much less in the hollow of the hand only, but is or ought to be effected in a crucible by a strong fire.

As to the combinations by precipitation, in which metals are dissolved, it is farther to be observed, that a fix'd alkaline salt, during its union with an acid, causes some alteration in the precipitated metal, which is frequently so considerable, that such metals, as are precipitated with it, are with greater difficulty reduced into their pristine malleable and fluxile, or fusible metallic form; which is sufficiently confirmed by the precipitated iron, in the preparation of *tartarus vitriolatus Tachenii*, upon the commixture of martial vitriol with a fix'd alkaline salt.

The fix'd alkaline salts are likewise wont to dissolve, besides those bodies already mentioned, other quite different metallic bodies, tho' some degree of causticity be required for this purpose. They dissolve the *calx* of lead, or even small filings of lead, the small filings of copper, or even copper precipitated from vitriol, as also the copper contained in verdigrease; they

alſo diſſolve tin, *regulus* of antimony, with which they are changed into a *calx*, nay laſtly, in a very ſtrong fire, into glaſs of an amber-colour; they diſſolve iron two ways; either, 1. When detonated with nitre, and a cauſtic nitre is thereby produced; and 2. Upon pouring a pure ſolution of fix'd alkaline ſalt upon a ſolution of iron made with ſpirit of nitre; as may be ſeen at greater length in a diſſertation publiſhed under M. *Stahl*.

But if M. *Neuman*'s fix'd alkaline ſalt be previouſly well impregnated with ſulphur, it will then diſſolve all metals (and likewiſe ſome ſemi-metals) namely copper, iron, lead, *regulus* of antimony, nay even tin and gold.

In fine, it is worth obſerving, that fix'd alkaline ſalts give a dry conſiſtence to alkalino-volatile ſalts, which alkaline earthy bodies do not; theſe fix'd ſalts likewiſe diſſolve the ſuperfluous oily ſubſtance, and which only adheres to the ſurface both of urinous and inflammable ſpirits.

Several celeſtial Obſervations, made at Southwick *in the County of* Northampton; *by Mr.* George Lynn. Phil. Tranſ. N° 393. p. 66.

THE following obſervations were made at *Southwick*, Long. weſt from *London* 30 minutes, Lat. 51 deg. 58 min. nearly, with a 13 foot teleſcope, whoſe aperture was 2.4 inches, and charge 2.5 inches; all by apparent time.

Nov. 8. 1724, $7^h\ 37'\ 7''$ the firſt ſatellite of *Jupiter* began to emerge; the ſame day at $6^h\ 24'\ 20''$ the third ſatellite began to immerge.

July 31, 1725, $10^h\ 43'\ 20''$ the third ſatellite immerged, that is Mr. *Lynn* quite loſt ſight of it (at a little above a ſemidiameter from *Jupiter*) but it began ſenſibly to abate of its light upwards of 3′ before.

Aug. 9. $11^h\ 51'\ 20''$, he loſt ſight of the ſecond ſatellite; but it began ſenſibly to abate of its light, about 2′ before.

Aug. 18. $9^h\ 25'\ 50''$, the firſt ſatellite immerged very near *Jupiter*'s body.

The ſame night both Mr. *Lynn* and his ſon plainly obſerved the ſhadow of the third ſatellite paſs over *Jupiter*'s body like a ſmall black patch, tracing along the middle of his bright belt, above the moſt ſouthern black one, and was in his axis, as near as Mr. *Lynn* could conjecture by the eye, at $10^h\ 25'$ or 30′.

N. B. They could ſee it for about the midde half of its track, but not near *Jupiter*'s edges.

Oct. 11.

Oct. 11. $6^h\ 31'\ 45''$, the third ſatellite began to emerge, and vas full $3'$ and a half, before it was at its greateſt luſtre, which Ir. *Lynn* could then well judge of, by comparing it with the rſt ſatellite, which was juſt a little above it, but nearer *Jupi-cr*: It emerged out of the ſhadow, about half a diameter from *Jupiter*'s limb.

Dec. 26. $5^h\ 51'\ 13''$ the ſecond ſatellite began to emerge.

January 5, 1725-6, $6^h\ 28'\ 30''$, the third ſatellite began to merge.

June 23, 1724. $10^h\ 15'$, *Saturn* followed a ſtar (in *Senex*'s odiac, but without any diſtinguiſhing mark $51''$ and $\frac{1}{2}$ of R. *Aſcenſ.* in time and $40''$ S. Declin.

June 25. 10^h *Saturn* followed the ſame ſtar $13''$ of *R. Aſcenſ.* n time and $3''$ or $4''$ at moſt S. Declin.

Dec. 17. 1725, 8^h *Jupiter* preceeded φ of *Aquarius* $4''$ and *R. Aſcenſ.* in time and $11'\ 45''$ S. Declin.

N. B. When two of *Jupiter*'s ſatellites are paſſing by each ther, the one approaching and the other receding from him (if ot too far diſtant from his body) the time, when they become qually diſtant from his limb, may by the eye, be very nearly etermined; eſpecially when the firſt and ſecond paſs in this anner, as Mr. *Lynn* by experience found by the above-entioned glaſs, within leſs than $\frac{1}{2}$ a min. in time, by the greement of two good obſervers.

The taking, therefore, the time of thoſe tranſits, namely, of e firſt and ſecond ſatellites, would be of more uſe in ſettling e longitude of places, than the eclipſes of any of the ſatellites xcepting the firſt; by reaſon of the length of time they take emerging or immerging, according to theſe obſervations.

An extraordinary High Tide *in the* River *of* Thames, *obſerved by Capt.* Jones; *together with Obſervations on the* Tides *in the ſaid* River; *by Mr.* Henry De Saumarez. Phil. Tranſ. N° 393. p. 68.

MARCH 8. 1725-6, the tide in the river of *Thames*, at *New Crane* in *Shadwell*, flowed 20 foot 5 inches and $\frac{1}{2}$, aken by a level from that high water mark, to low water next orning; and was 4 inches higher than has been known theſe o years.

That the uſe of Mr. *De Saumarez*'s inſtrument (vide *Phil. Tranſ.* N° 391) called the *marine-ſurveyor*, may yet farther ppear, he here gives ſome experiments he made with it on the

the river of *Thames*, in order to determine the strength of the tides of flood and ebb. Were the same to be done in the *Channel*, and on the sea coast of *Great Britain*, and marked in our charts, he thinks, it would be of no small advantage to our commerce; and consequently, a sufficient recommendation of the *marine-surveyor*, if that alone were the use of it.

He is the rather induced to be of this opinion, in regard he is not insensible of the dangers on the *Casquets*, in the *race* of *Alderney*, &c. where rapid tides and currents have occasioned too many losses. As Mr. *De Saumarez* (to the manifest hazard of his life) surveyed and took correct draughts not only of these, but of the islands of *Guernsey*, *Sarck*, &c. and as he persuades himself they are as correct, as any thing that has hitherto appeared of this kind, he has published them for the good of the publick.

A table shewing the strength, and gradual increase and decrease of the tides of flood and ebb in the river of *Thames*, as observed in *Lambeth* reach, off of *Manchester* stairs, and in the middle of the river, with a new instrument, called the *marine-surveyor*, on the 9. of *June*, 1720; it being then full moon, and consequently, a spring tide; the movement of the machine being 14 inches under water.

EBB

FLOOD.

The Time of Flood.		The Depth of the River.		The Run of the Curr. in 15 Min	The whole Run of the Curr. to the Times expr. in the first Column.	The same reduc'd to Stat. Miles of 5280 Ft, or 528 Revolutions of the Machine.			The Reduction into English maritime Miles of 6000 Feet, or 600 Revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts	Rev.	M.	Pts.	Rev.
	15	5		110	110			11			11
	30	6		590	700			70			70
	45	6	9	1100	1800		1:4	48		1:4	30
1		7		1490	3290		1:2	55		1:2	29
1	15	8		1870	5160		3:4	120		3:4	66
	30	9		2230	7390	1	1:4	79	1		139
	45	10		2500	9890	1	3:4	65	1	1:2	89
2		11	6	2660	12550	2	1:4	67	2		55
2	15	13		2730	15280	2	3:4	76	2	1:2	28
	30	14		2740	18020	3	1:4	86	3		2
	45	14	9	2720	20740	3	3:4	94	3	1:4	124
3		14	9	2570	23310	4	1:4	87	3	3:4	81
3	15	14	10	2220	25530	4	3:4	45	4	1:4	3
	30	14	9	1820	27350	5		66	4	1:2	35
	45	14		.990	28340	5	1:4	62	4	1:2	134
	50	13	9	.130	28470	5	1:4	75	4	1:2	147

EBB.

The Time of Ebb.		The Depth of the River.		The Run of the Current in every 15 Min.	The whole Run of the Current to the Times expressed in the first Column.	The same reduced to Statute Miles of 5280 Feet, or 528 Revolutions of the Machine.			The Reduction into English maritime Miles of 6000 Feet, or 600 Revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts.	Rev.	M.	Pts.	Rev.
	15	12	9	280	280			28			28
	30	12	3	1140	1420		1:4	10			142
	45	11	10	1900	3320		1:2	68		1:2	32
1		11	4	2080	5400	1		12		3:4	90
1	15	11	2	2120	7520	1	1:4	92	1	1:4	2
	30	10	9	2120	9640	1	3:4	40	1	1:2	64
	45	10	4	2170	11810	2		125	1	3:4	131
2		10		2130	13940	2	1:2	74	2	1:4	44
2	15	9	6	2060	16000	3		16	2	1:2	100
	30	9	4	2040	18040	3	1:4	88	3		4
	45	9		2020	20060	3	3:4	26	3	1:4	56
3		8	9	1910	21970	4		85	3	1:2	97

The Time of Ebb.		The Depth of the River.		The Run of the Current in every 15 Min	The wholeRun of the Current to the Times expressed in the first Column.	The same reduced to Statute Miles of 5280 Feet or 528 Revolutions of the Machine.			The Reduction into English Maritime Miles of 6000 Feet or 600 Revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts.	Rev.	M.	Pts.	Rev.
3	15	8	6	1900	23870	4	1:2	11	3	1:4	137
	30	8	3	1910	25780	4	3:4	70	4	1:4	28
	45	8		1860	27240	5		124	4	1:2	64
4		7	9	1810	29450	5	1:2	41	4	3:4	95
4	15	7	3	1780	31230	5	3:4	87	3		123
	30	7		1690	32920	6		124	5	1:4	142
	45	6	6	1620	34540	6	1:2	22	5	3:4	4
5		6	3	1570	36110	6	3:4	47	5		11
5	15	6	3	1570	37680	7		72	6	1:4	18
	30	6		1570	39250	7	1:4	97	6	1:2	25
	45	6		1560	40810	7	1:2	121	6	3:4	31
7		5	9	1550	42360	8		12	7		36
6	15	5	6	1500	43860	8	1:4	30	7	1:4	36
	30	5	3	1460	45320	8	1:2	44	7	1:2	32
	45	5		1450	46770	8	3:4	57	7	3:4	27
6		4	9	1430	48200	9		68	8		20
7	15	4	6	1400	49600	9	1:4	76	8	1:4	10
	30	4	3	1380	50980	9	1:2	82	8	1:4	148
	45	4	3	1340	52320	9	3:4	84	8	1:2	232
8		4		1270	53590	10		79	8	3:4	109
8	5	3	10	420	54010	10		121	9		1
	10	3	11	410	54420	10	1:4	30	9		42
	15	4		400	54820	10	1:4	70	9		82
	20	4		380	55200	10	1:4	108	9		120
	25	4	2	300	55500	10	1:2	6	9	1:4	
	30	4	2	270	55770	10	1:2	33	9	1:4	27
	35	4	3	130	55900	10	1:2	46	9	1:4	40
	40			Stagn.	Stagnant.						

A table ſhewing the length and gradual increaſe of the tides of flood and ebb in the river of *Thames*, as obſerved in *Lambeth* reach, off of *Mancheſter* ſtairs, and in the middle of the river, with a new inſtrument, call'd the *Marine-Surveyor*, on the 18. of *June*, 1720; it being then the laſt quarter of the moon, and conſequently a neap tide; the movement of the machine being 14 inches under water.

FLOOD.											
The time of the Flood.		The Depth of the River.		The Run of the Current in every 15 Min	Thewhole Run of the Current to the Times expreſſed in the firſt Column.	The ſame reduced to Statute Miles of 5280 Feet, or 528 Revolutions of the Machine.			The Reduction into Engliſh Maritime Miles of 6000 Feet, or 600 Revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts.	Rev.	M.	Pts.	Rev.
	15	4		220	220			22			22
	30	4	3	520	740			74			74
	45	4	9	900	1640		1:4	32		1:4	14
1		5	3	1030	2670		1:2	3		1:4	117
1	15	5	9	1020	3690		1:2	105		1:2	69
	30	6	1	1160	4850		3:4	89		3:4	35
	45	7		1450	6300	1		102	1		30
2		7	9	1640	7940	1	1:2	2	1		44
2	15	8	1	1830	9770	1	3:4	53	1	1:2	77
	30	9		1920	11690	2		113	1	3:4	119
	45	9	6	2070	13760	2	1:2	56	2	1:4	26
3		10		2170	15930	3		9	2	1:2	
3	15	10	4	2070	18000	3	1:4	84	3		
	30	11	3	1960	19960	3	3:4	16	3	1:4	46
	45	11	4	1890	21850	4		73	3	1:2	85
4		11	9	1700	23550	4	1:4	111	3	3:4	105
4	15	11	6	1300	24850	4	1:2	109	4		85
	30	11		730	25580	4	3:4	50	4	1:4	8
	35	11		70	25650	4	3:4	57	4	1:4	15
	40			Stagn.	Stagnant.						
	45	10	10	dit.	dit.						
	50	10	9	dit.	dit.						

E B B.											
The time of ebb.		The depth of the river.		The run of the current in every 15 min.	The whole run of the current to the times expressed in the first column.	The same reduced to statute miles of 5280 feet, or 528 revolutions of the *Machine*.			The reduction into *English* maritime miles of 6000 feet, or 600 revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts.	Rev.	M.	Pts.	Rev.
	15	10	6	610	610			61			61
	30	10		1340	1950		1:4	63		1:4	45
	45	9	9	1520	3470		1:2	83		1:2	47
1		9	3	1650	5120		3:4	116		3:4	62
1	15	9		1750	6870	1	1:4	27	1		87
	30	8	6	1730	8600	1	1:2	68	1	1:4	110
	45	8		1700	10300	1	3:4	106	1	1:2	130
2		7	9	1710	12010	2	1:4	13	2		1
2	15	7	3	1710	13720	2	1:2	52	2	1:4	22
	30	7	1	1710	15430	2	3:4	91	2	1:2	45
	45	6	9	1710	17140	3		130	2	3:4	64
3		6	7	1680	18820	3	1:2	34	3		82
3	15	6	4	1670	20490	3	3:4	69	3	1:4	99
	30	6		1570	22060	4		94	3	1:2	106
	45	5	9	1500	23560	4	1:4	112	3	3:4	106
4		5	8	1480	25040	4	1:2	128	4		104
4	15	5	3	1440	26480	5		8	4	1:4	98
	30	5	2	1430	27910	5	1:4	19	4	1:2	91
	45	5		1420	29330	5	1:2	29	4	3:4	83
5		5		1430	30760	5	3:4	40	5		76
5	15	4	10	1420	32180	6		50	5	1:4	68
	30	4	6	1430	33610	6	1:4	61	5	1:2	61
	45	4	4	1420	35030	6	1:2	71	5	3:4	53
6		4	1	1380	35410	6	3:4	77	6		41
6	15	3	11	1360	37770	7		81	6	1:4	27
	30	3	11	1340	39110	7	1:4	83	6	1:2	11
	45	3	10	1230	40340	7	1:2	74	6	1:2	134
7		3	10	1070	41410	7	3:4	49	6	3:4	91
7	15	3	11	530	41940	7	3:4	102	6	3:4	144
	20	4		20	41960	7	3:4	104	6	3:4	146
	25			Stagn.	Stagn.						
	35	4	3	dit.							

A new and exact Table *collected from several Observations, taken in four Voyages to* Hudson's Bay *in North* America, *from* London: *Shewing the Variation of the* Magnetical Needle, *or* Sea Compass, *in the Path-way to the said* Bay, *according to the several* Latitudes *and* Longitudes, *from the Year* 1721, *to* 1725; *by Capt.* Christopher Middleton. Phil. Trans. N° 393. p. 73.

Lat.		Long.		Vari.		Lat.		Long.		Vari.	
D.	M.	D.	M.	D.	M.	D.	M.	D.	M.	D.	M.
50	00	12	00	14	00	50	00	18	45	17	00
51	00	12	00	14	15	51	00	18	45	17	15
52	00	12	00	14	30	52	00	18	45	17	30
53	00	12	00	14	45	53	00	18	45	17	45
54	00	12	00	15	00	54	00	18	45	18	00
55	00	12	00	15	15	55	00	18	45	18	15
56	00	12	00	15	30	56	00	18	45	18	30
57	00	12	00	15	45	57	00	18	45	18	45
58	00	12	00	16	00	58	00	18	45	19	00
59	00	12	00	16	15	59	00	18	45	19	15
50	00	14	15	15	00	50	00	21	00	18	00
51	00	14	15	15	15	51	00	21	00	18	15
52	00	14	15	15	30	52	00	21	00	18	30
53	00	14	15	15	45	53	00	21	00	18	45
54	00	14	15	16	00	54	00	21	00	19	00
55	00	14	15	16	15	55	00	21	00	19	15
56	00	14	15	16	30	56	00	21	00	19	30
57	00	14	15	16	45	57	00	21	00	19	45
58	00	14	15	17	00	58	00	21	00	20	00
59	00	14	15	17	15	59	00	21	00	20	15
50	00	16	30	16	00	50	00	23	15	19	00
51	00	16	30	16	15	51	00	23	15	19	15
52	00	16	30	16	30	52	00	23	15	19	30
53	00	16	30	16	45	53	00	23	15	19	45
54	00	16	30	17	00	54	00	23	15	20	00
55	00	16	30	17	15	55	00	23	15	20	15
56	00	16	30	17	30	56	00	23	15	20	30
57	00	16	30	17	45	57	00	23	15	20	45
58	00	16	30	18	00	58	00	23	15	21	00
59	00	16	30	18	15	59	00	23	15	21	15

Lat.		Long.		Vari.		Lat.		Long.		Vari.	
D.	M.	D.	M.	D.	M.	D.	M.	D.	M.	D.	M.
50	00	25	30	20	00	50	00	34	30	24	00
51	00	25	30	20	15	51	00	34	30	24	15
52	00	25	30	20	30	52	00	34	30	24	30
53	00	25	30	20	45	53	00	34	30	24	45
54	00	25	30	21	00	54	00	34	30	25	00
55	00	25	30	21	15	55	00	34	30	25	15
56	00	25	30	21	30	56	00	34	30	25	30
57	00	25	30	21	45	57	00	34	30	25	45
58	00	25	30	22	00	58	00	34	30	26	00
59	00	25	30	22	15	59	00	34	30	26	15
50	00	27	45	21	00	50	00	36	45	25	00
51	00	27	45	21	15	51	00	36	45	25	15
52	00	27	45	21	30	52	00	36	45	25	30
53	00	27	45	21	45	53	00	36	45	25	45
54	00	27	45	22	00	54	00	36	45	26	00
55	00	27	45	22	15	55	00	36	45	26	15
56	00	27	45	22	30	56	00	36	45	26	30
57	00	27	45	22	45	57	00	36	45	26	45
58	00	27	45	23	00	58	00	36	45	27	00
59	00	27	45	23	15	59	00	36	45	27	15
50	00	30	00	22	00	50	00	39	00	26	00
51	00	30	00	22	15	51	00	39	00	26	15
52	00	30	00	22	30	52	00	39	00	26	30
53	00	30	00	22	45	53	00	39	00	26	45
54	00	30	00	23	00	54	00	39	00	27	00
55	00	30	00	23	15	55	00	39	00	27	15
56	00	30	00	23	30	56	00	39	00	27	30
57	00	30	00	23	45	57	00	39	00	27	45
58	00	30	00	24	00	58	00	39	00	28	00
59	00	30	00	24	15	59	00	39	00	28	15
50	00	32	15	23	00	50	00	41	15	27	00
51	00	32	15	23	15	51	00	41	15	27	15
52	00	32	15	23	30	52	00	41	15	27	30
53	00	32	15	23	45	53	00	41	15	27	45
54	00	32	15	24	00	54	00	41	15	28	00
55	00	32	15	24	15	56	00	41	15	28	15
56	co	32	15	24	30	57	00	41	15	28	30
57	00	32	15	24	45	58	00	41	15	28	45
58	00	32	15	25	00	59	00	41	15	29	00
59	00	32	15	25	15						

Lat.

Lat.		Long.		Vari.	
D.	M.	D.	M.	D.	M.
50	00	43	30	28	00
51	00	43	30	28	15
52	00	43	30	28	30
53	00	43	30	28	45
54	00	43	30	29	00
55	00	43	30	29	15
56	00	43	30	29	30
57	00	43	30	29	45
58	00	43	30	30	00
59	00	43	30	30	15
51	00	46	00	29	00
52	00	46	00	29	15
53	00	46	00	29	30
54	00	46	00	29	45
55	00	46	00	30	00
56	00	46	00	30	15
57	00	46	00	30	30
58	00	46	00	30	45
59	00	46	00	31	00
52	00	48	30	30	00
53	00	48	30	30	15
54	00	48	30	30	30
55	00	48	30	30	45
56	00	48	30	31	00
57	00	48	30	31	15
58	00	48	30	31	30
59	00	48	30	31	45
53	00	51	00	31	00
54	00	51	00	31	15
55	00	51	00	31	30
56	00	51	00	31	45
57	00	51	00	32	00
58	00	51	00	32	15
59	00	51	00	32	30
60	00	51	00	32	45
54	00	54	00	32	00
55	00	54	00	32	15
56	00	54	00	32	30
57	00	54	00	32	45
58	00	54	00	33	00
59	00	54	00	33	15
60	00	54	00	33	30
61	00	54	00	33	45

Lat.		Long.		Vari.	
D.	M.	D.	M.	D.	M.
54	00	57	00	33	00
55	00	57	00	33	15
56	00	57	00	33	30
57	00	57	00	33	45
58	00	57	00	34	00
59	00	57	00	34	30
60	00	57	00	35	00
61	00	57	00	35	30
55	00	60	00	34	00
56	00	60	00	34	30
57	00	60	00	35	00
58	00	60	00	35	30
59	00	60	00	36	00
60	00	60	00	36	30
61	00	60	00	37	00
57	00	63	00	35	00
58	00	63	00	35	30
59	00	63	00	36	00
60	00	63	00	36	30
61	00	63	00	37	00
62	00	63	00	37	30
59	00	66	00	37	00
60	00	66	00	37	40
61	00	65	00	38	20
62	00	66	00	39	00
63	00	66	00	39	40
60	00	69	00	41	00
61	00	69	00	41	40
62	00	69	00	42	20
60	00	72	00	40	00
61	00	72	00	42	00
61	40	72	00	42	40
62	00	78	00	43	00
63	00	78	00	44	00
63	50	78	00	46	00

Lat.		Long.		Vari.	
D.	M.	D.	M.	D.	M.
61	00	75	00	38	00
62	00	75	00	43	00
62	00	75	00	45	00
63	00	81	00	43	00
64	00	81	00	46	00
62	00	82	00	39	00
63	00	82	00	44	00
61	00	84	00	33	45
62	00	84	00	40	00
63	00	84	00	42	00
60	00	86	00	30	00
61	00	86	00	33	00
62	00	86	00	35	00
59	00	88	00	28	00
60	00	88	00	28	40
61	00	88	00	29	20
57	00	90	00	24	00
58	00	90	00	24	30
59	00	90	00	25	00
57	00	94	00	23	00
58	00	95	00	22	30
59	00	95	00	21	00

From long. 68 to long. 81 degrees in *Hudson's Bay* is found the greatest variation, and where the compass will hardly traverse.

An Account of several Experiments (some of which were made before the Royal Society*) concerning the Running of Water in* Pipes, *as it is retarded by Friction, and intermix'd* Air; *together with the Description of a new* Machine, *by which Pipes may be clear'd of* Air, *as the Water runs along, without* Stand-pipes, *or the Help of any Hand; by Dr.* Desaguliers. Phil. Trans. N° 393. p. 77.

DR. *Desaguliers* having found by several experiments in small, that water would not be discharged in the same quantity by a great deal, thro' a long pipe, as thro' a shorter of the same bore, the orifice being at the same depth under the surface of the water in a reservoir; he made an experiment upon a pipe upwards of 1000 yards in length; and of one and three quarters of an inch bore; and found, that the quantity of water given was much less (he thinks by $\frac{11}{12}$) than

than it ſhould have been, according to M. *Mariotte*'s rules; and that ſomething more than the friction, on account of the length of the pipe, had retarded the water; which he afterwards found to be air confined in the eminent parts of the pipe. Some years before he publiſhed a full account of this experiment, in his notes upon *Mariotte's Mouvement des eaux* in the *Engliſh* tranſlation.

Conſidering this matter again, he made the following experiment; A (Fig. III. Plate 3.) is a veſſel, containing a cubic foot in the inſide, and always kept full by means of the pipe B, running from a larger veſſel; C D a ſhort pipe of $\frac{3}{4}$ of an inch bore, two foot in length, opening into the bottom of the ciſtern A, and whoſe orifice D is always 10 inches below the bottom of A.

OGEEEHI repreſents another pipe of the ſame bore, whoſe orifice F is likewiſe 10 inches below the bottom of A: This pipe is 113 yards long, lying along the ground, five foot below A, except the depending part O G, and the aſcending part H F. When F is ſtopped, and (A being kept full) the water runs out at D, the quantity of water given is 19 times more than when D is ſtopped, and the water runs out at F. The air, confined in ſeveral parts of the long pipe, is the chief reaſon of this difference.

In order to get rid of the air, which, lodging in the pipe, contracts its bore; and thereby leſſens the quantity of water, to be delivered at the iſſue; he made ſeveral experiments to find whereabouts the air lodges, the more eaſily to let it out; one of which was, as follows.

He took a glaſs-pipe, as A B (repreſented Fig. 2.) about an inch in diameter, 12 foot in length from P to P; only the parts A P and P B at the other end were of lead; then pouring in water at A, till it came up to B (ſtopping the extremity G) the air lodged in the eminent parts of the pipe, at the places mark'd C C, D D and E E: But when the water was ſuffered to go out at G, the air came forwards towards G, and took up the ſpaces *cc*, *dd*, and *ee*, contracting the bore of the pipe as before, but it ſtood more forwards in the pipe: So that it generally happened, that the ſpace of air began on the upper part of the eminence of the pipe.

N. B. The glaſs pipe may be made of ſeveral pieces, joined to each other, and to the leaden pipes and funnels, by braſs ferrils and elbows, turning in all manner of angles: Theſe are not repreſented in the figure.

If

If the velocity of the water be very great, the air will go even beyond the eminence of the pipe.

To let out from the conduct pipes the air, that obstructs the running of the water, the Dr. recommends the experiments he made, and the *apparatus* he applied to a wooden conduit pipe of 9 inches bore, which runs a mile and a half from the water-engine at *York-buildings* to a reservoir near *Cavendish-square*; the surface of the water in the cistern at the water house being sometimes 15, and sometimes 20 foot above the issue at the reservoir.

Upon a part of the pipe, as A B (Fig. 3.) he fixed a leaden pipe D F of two inches bore, by means of three ferrils, or short communication-pipes; the first at D, just beyond the beginning of the space C C, that used to be filled with air in the running of the water; the second in the middle of the leaden pipe, and the third at the end of it; the length of the pipe itself being from 12 to 24 feet, according to the steepness of the descent, the shortest pipe being sufficient, where the descent is very quick: From the middle of the aforesaid leaden pipe (called a *rider*, from its being laid along on the main or conduct pipe) there goes another pipe, as E H, of the same diameter, rising all the way very gently from E to the cock H, and so on to I; because if there were the least descent, water would lodge in it.

Now, when the water runs from A to B, the first ferril D will catch the air, as it runs; so as to let it out at I, if the cock H be open, sometimes without going to G or to C. But if the cock had not been opened, till the water had passed thro' the part A B of the pipe, the air would lodge in the space C C, and be discharged upon the opening of the cock. After the cock has been shut, when no more air comes, and water succeeds, after some time, air will extricate itself out of the water, and come up to C C; or if it come from the part of the pipe towards B, it will rise contrary to the current of the water quite up to C; and so go out at the pipe E H, when the cock is opened again.

As after the first discharge of the air, it cannot be known when more air is got into the pipe, unless by opening the cock; which would require one man constantly to attend each cock, and occasion a waste of water, at every turn of the cock, unless when air happens to be in the pipe; it was proposed to contrive a valve that should open to let out the air, and shut again when the water came; and an inverted brass clack or valve shutting upwards,

upwards, and falling down by its own weight, with cork fix'd to its under ſide, to help it to riſe, when the water came, was mentioned, as fit for the purpoſe by ſome of the perſons the Dr. was talking with about it; but that propoſal was rejected; becauſe when ſuch a valve has been ſhut ſome time, if air ſhould extricate itſelf from the water, it would be denſe air, whoſe force being equal to that of a column of water 30, 60, 80, or more feet in height, it would keep the valve ſhut, as well as the water did before, tho' the air at firſt could not ſhut the ſaid valve.

At laſt, after ſeveral thoughts, a machine was contrived, which exactly anſwers the purpoſe, and is very ſimple; therefore, it will be of general uſe: The deſcription of it is, as follows.

G (Fig. 4.) repreſents a ſection of the main or conduct pipe, with water up to G, and air above it; A B being an horizontal line, touching the top of the ſaid pipe; E H I is the leaden pipe deſcribed above, and marked with the ſame letters, as in Fig. 3. reaching from the pipe in the ſtreet to the ſide of a houſe, or to the ſide of one of the poſts, ſet up to keep off coaches from the foot-path; the machine is the box K made of caſt iron, fixed to the leaden pipe at I, with a thin door of plate iron, moving on hinges, and made to lock at D. This box ſtands in the ſtreet out of the way of paſſengers, with its bottom fixed to a plank in the pavement, ſo as not to be damaged by a ſmall ſhock, or any chance blow. The ſeveral parts of the machine, are as follows.

N N (Fig. 5.) is an iron plate about an inch thick, with four holes at 1, 2, 3, 4, of about an inch in diameter, quite thro' the plate, to let thro' four ſcrews, ſuch as *a*; O O is a face, or flat ring raiſed out of the whole ſtuff, and prominent about $\frac{1}{4}$ of an inch, ground, or turned to a true flat; 5, is a hole of about 1 inch and $\frac{1}{2}$ in diameter, to receive the noſe of a cock, which is put thro' it, ſtopping with a ſhoulder or flaunch, ſcrew'd within the circle O O, by four other ſcrews, mark'd with large points round the hole 5.

N N (Fig. 6.) repreſents the ſame plate edge-wiſe; M the air-cock ſcrew'd to the ſaid plate, thro' the flaunch of its pipe at *m n*, having its key 6, 10 faſtened to a rod of about $\frac{1}{2}$ an inch in diameter of the figure 6, 7, 8, 10, with a ſhank 1 foot long 8, 9, joined to a buoy or hollow copper-ball L, which ball, when the ſaid ſhank is in an horizontal ſituation, keeps the cock ſhut; but falling by its own weight, when not ſuſtained by the

the water, opens the cock by means of the rod 8, 9, as may be seen in Fig. 7, where the plate NN is screw'd to the box; and the prick'd line ML shews the surface of the water, coming into the box thro' the great cock, and leaden pipe HI; so as to cause the ball L float with its shank in the horizontal situation 8, 9; but when more air comes in to drive the water down the pipe I, the buoy will fall to *l*, and its shank, coming down to 10, 11, will open the air-cock M, and let out the air (be its density what it will) till it be all discharged, and the water is again got up to LM, and has raised up the buoy to L; NN is the fore-part of the box with its hole, to which the plate of Fig. 5. is screwed.

It is easily conceived, that the cock H must always be left open; that the end of the pipe I is screwed to a hole in the bottom of the box, by means of screws at *rr*; that there are oil'd leathers at the heads of all the screws, and likewise upon the plate NN, to make the face OO of Fig. 5. apply itself close to the fore-part of the box K (Fig. 8.) which has a hole at OO to take in the buoy and cock of Fig. 6. the screws at 1, 2, 3, 4, which have their heads within the box, and their nuts, such as *b* (Fig. 5.) screwed on, when the plate NN is applied; and that the whole box, thus fitted, is made air-tight.

D in Fig. 4. and D in Fig. 7. represent an iron door to cover the mouth of the air-cock from external injury; and it is punched full of holes, to let out the air freely.

This machine, which from its make, is called a *Jack in a box*, will be useful, wherever water is to be conveyed a great way in pipes. The box is the joint invention of Dr. *Desaguliers*, Mr. *Richard Jones*, Mr. *James King*, Mr. *Thomas Newcomen*, Mr. *Joseph Hornblower*, and his operator.

The Longitude of Lisbon *and the* Fort *of* New York *from* Wansted *and* London, *determined by Eclipses of the first Satellite of* Jupiter; *by Mr.* James Bradley. Phil. Trans. N° 394. p. 85.

SOME curious astronomical observations, having been communicated to the *Royal Society*, among which were several eclipses of *Jupiter*'s first satellite; Mr. *Bradley* examined whether he had made any at *Wansted*, which tallied with them, that by comparing such together, the true difference of Long. between those places might be found: But looking over his observations of the first satellite made in the year 1725 and beginning of 1726, he met only with two emersions, that were observed

obſerved the ſame night, both at *Lisbon* and *Wanſted*. There are others, it is true, made within a few days of each other, which may likewiſe be made uſe of to determine the difference of longitude; but not with the ſame degree of certainty, by reaſon of the irregular motion of the ſatellite, which, he preſumes, does chiefly ariſe from the gravitation of the other ſatellites towards it. For, tho' the effect of the influence the ſatellites have on each other, is moſt remarkable in the ſecond, whoſe motion will ſometimes be accelerated or retarded thereby, as much as amounts to 30 or 40 minutes in time, in the ſpace of about ſeven months, or in half the period, in which the three innermoſt ſatellites return to have nearly the ſame poſition with reſpect to each other and the ſhadow of *Jupiter*; yet the firſt ſeems alſo liable to inequalities, that cannot well be accounted for, but from ſome ſuch cauſe as is before-mentioned, the effect of which will not eaſily be reduced to any rule, but from a long and exact ſeries of obſervations; and till ſome better and more certain rule can be found out, we may ſuppoſe, that the effect produced by this cauſe is, during ſmall intervals, proportionable to the time. On this ſuppoſition he compared ſome obſervations with others, not made the ſame nights; and the reſult is nearly the ſame, as in thoſe which were obſerved at the ſame time in both places, as will appear by the following particulars.

The immerſion of the firſt ſatellite was obſerved at *Wanſted* with Mr. *Hadley*'s reflecting teleſcope on *Auguſt* 4. 1725, N. S. about 45″ after the time of the immerſion, as calculated from Mr. *Bradley*'s tables: By another obſervation made *Aug.* 29. N. S. the true immerſion preceeded the calculation from the ſame tables 1′ 10″: So that in 25 days the ſatellite's motion was accelerated, as much as anſwered to 1′ 55″ in time. Suppoſing, therefore, the acceleration to have been in the ſame proportion between *July* 28, and *Auguſt* 4. N. S. then the true immerſion *July* 28. N. S. would have happened at *Wanſted* about 1′ 15″ after the time by the tables, which make the immerſion at 12^h 48′ 45″ apparent time; the true immerſion, therefore, was at *Wanſted* *July* 28. 12^h 50′ apparent time; and at *Lisbon* it was obſerved at 12^h 12′ 26″ apparent time; the difference being 37′ 34″.

Sept. 28. N. S. the firſt ſatellite was ſeen emerging in the reflecter at *Wanſted* 3′ 50″ ſooner than the tables make the emerſion; and by the mean of two more obſervations made at the

 ſame

ſame place, and with the ſame teleſcope, on the 14. and 16. of *October*, N. S. the true emerſion preceded the calculation by the tables about 3′ 35″; and that the true emerſion there was at 12^h 1′ 15″ *April* 1. but this emerſion was obſerved at *Lisbon* at 11^h 24′ 55″; the difference being 36′ 20″.

The obſervations at *Wanſted* being made with Mr. *Hadley's* reflecting teleſcope (by which one may ſee the firſt ſatellite near ¼ of a minute ſooner when it is emerging, than in a refracting teleſcope of 15 feet; and the contrary when it is immerging) there ought to be ſome allowance made on account of different teleſcopes made uſe of at *Lisbon* and *Wanſted*, by deducting 10 or 15″ from the difference of time, collected from the immerſions, and adding as much to the difference, deducted from the emerſions. Such correction being made, the difference of meridians by the immerſion obſerved *July* 28. will be 37′ 20″, and by the emerſion *Sept*. 21, 36′ 35″.

The emerſion, obſerved at *Lisbon Dec*. 8. N. S. at 8^h 32′ 40″ apparent time, was likewiſe obſerved at *Wanſted* in a 15 foot teleſcope at 9^h 10′ 5″ apparent time, the air being ſomewhat hazy, which may probably make the difference 37′ 25″ a little too great.

The emerſion obſerved at *Lisbon Jan*. 16. 1726, N. S. at 6^h 51′ 10″, which ſeems accompanied with circumſtances that argue its exactneſs, was likewiſe very well obſerved at *Wanſted* in a 15 foot teleſcope, at 7^h 28′ 22″ apparent time, the difference being 37′ 12″.

Theſe are the only obſervations among thoſe which were laſt communicated, that Mr. *Bradley* could compare with any degree of certainty with his own: But he likewiſe finds others printed in the *Phil. Tranſ*. N° 385. which were likewiſe made by the ſame curious perſons, who obſerved an emerſion of the firſt ſatellite at *Lisbon Sept*. 2. 9^h 36′ 57″ 1724, N. S. This was alſo obſerved at *Wanſted* in the reflecter at 10^h 13′ 28″ app. time. Hence allowing for the different teleſcopes, the difference of meridians is 36′ 45″.

This emerſion at *Wanſted* preceeded the calculation by the tables 4′ 40″; and another emerſion, obſerved with the ſame teleſcope on *Sept*. 18. N. S. preceeded the calculation 5′ 10″. We may, therefore, ſuppoſe, that on *Sept*. 9. N. S. the true emerſion at *Wanſted* preceeded the computed emerſion about 4′ 52″. The emerſion that day by the tables was at 12^h 15′ 34″ app. time; the true emerſion, therefore, at *Wanſted* was at 12^h

10′ 42″

10′ 42″. At *Lisbon* it was obſerved at 11ʰ 34′ 26″: So that allowing for the difference of teleſcopes, the difference of meridians by this obſervation is 36′ 30″.

The mean of all theſe differences is about 36′ 58″, from which ſubſtracting 28″ for the difference of meridians between *London* and *Wanſted*, the remainder will be the difference of meridians between *London* and *Lisbon*, *viz.* 36′ and ½, *Lisbon* being ſo much to the weſtward of *London*. This difference of longitude is about 5′ and ½ greater than what is determined in the abovementioned *Tranſaction*.

The ſame *Tranſaction* containing ſome obſervations of eclipſes of the ſame ſatellite, made in the *Fort* of *New York*, and communicated by Mr. *Burnet*, the Governour, M. *Bradley* determines the longitude of that *Fort* more exactly than it can be ſuppoſed to be there done, by the bare compariſon of the obſervations with the tables; having two obſervations made at *Wanſted*, which tally with two made at *New York* on *Aug.* 25. and *Sep.* 10.

By the obſervation made *Aug.* 25. 1723. O. S. which is ſuppoſed to be the diſtincteſt and beſt, the ſatellite emerged at 9ʰ 35′ 14″ by the clock, which went about 1′ and ¼ too faſt for the apparent time at the emerſion, as appears by the altitudes of the ſun's limb, taken the morning before and after the obſervation: So that the emerſion at *New York* was at 9ʰ 34′ app. time; that is 9ʰ 32′ 20″ mean time.

Auguſt 27, 8ʰ 57′ 40″ mean time, the ſatellite was obſerved emerging at *Wanſted* in the reflecter; and *Sept.* 12. 7ʰ 17′ 15″ mean time it was obſerved emerging again in the ſame teleſcope: So that in 15d. 22h. 19′ 35″ there were 9 emerſions; and the interval between each was about 1d 18h 28′ 50″; this ſubſtracted from the time of the emerſion, obſerved at *Wanſted Aug.* 27. will give the true emerſion at *Wanſted* on *Aug.* 25. 14h 28′ 50″ M. T. that is 4h 56′ 30″ later than it was obſerved at *New York*.

Sept. 10. 8h 0′ 10″, by the clock, another emerſion was obſerved at *New York*. From the altitudes of the ſun's limb, taken the morning before, Mr. *Bradley* computed the error of the clock at the time of the emerſion to be 1′ 10″; and that the emerſion was at 7h 59′ app. time, that is, 7h 51′ 52″ mean time at *New York*: But ſubſtracting the abovementioned interval of 1d 18h 28′ 50″ from the time of the emerſion, obſerved at *Wanſted*, *Sept.* 12, 7h 17′ 15″ M. T. we ſhall have the time of the true emerſion at *Wanſted* on *Sept.* 10. at 12h 48′ 25″ M. T.

 which

which is 4h 56′ 33″ later than it was observed at *New York*: The difference, therefore, of meridians between *Wansted* and *New York*, allowing about 15″ for the difference of telescopes, is about 4h 56′ 45″; and between *London* and *New York* 4h 56′ and ¼: So that the true longitude of *New York* from *London* is 74° 4′ west.

Eclipses *of* Jupiter's *first* Satellite *observed at* Lisbon *in the Year* 1725, 1726; *by F.* Carbone, *and at* Toulon *by F.* Laval. Phil. Transf. N° 394. p. 90. *Translated from the* Latin.

true time H.	M.	S.	1725	
12	12	26	*July* 28.	*Jupiter*'s innermost satellite was observed to immerge into the planet's true shadow.
12	11	35		Its light began to grow weak.
15	0	10	*Sept.* 12.	It emerged out of *Jupiter*'s true shadow, the sky being pretty clear; but by reason of the planet's opposition to the sun, which had happened seven days before, the satellite was so near his disk, that by reason of his extraordinary brightness it might be somewhat obscured at its first egress out of the shadow; and consequently, there may be some uncertainty as to few seconds.
9	28	7	14.	It began to emerge out of the shadow.
9	29	0		It recovered its light entirely.
11	24	55	21.	The beginning of the emersion out of the true shadow.
11	26	00		It recovered its light entirely.
8	11	10	*Oct.* 23.	The beginning of the emersion.
8	12	10		It recovered its light entirely.
6	30	4	*Nov.* 8.	The satellite was observed to grow bright in the *penumbra*, yet with some uncertainty, as to a few seconds, of the true beginning of the emersion, by reason of the vibration of the air by the wind.
8	24	50	15.	The beginning of the emersion was distinctly observed.
8	25	50		It recovered its light entirely.

true

true time				
H.	M.	S.		
8	33	40	*Dec*, 8.	The beginning of the emerſion.
8	33	30		It recovered its light entirely.
			1726	
4	58	50	*Jan.* 9.	At *Toulon* the firſt ſatellite of *Jupiter* was obſerved to grow clear, but with a very thin light, by reaſon of the clearneſs of the air from the *crepuſculum*; and conſequently the true beginning of the emerſion was uncertain, at leaſt as to ſeconds.
6	51	10	16.	It was firſt obſerved to emerge pretty diſtinctly, the air being quite calm and ſerene.
6	52	15		It recovered its light entirely.

By ſeveral very accurate obſervations the Lat. of *Toulon* is 43° 6′ 55″.

The Latitude of Lisbon; *by F.* Carbone. Phil. Tranſ. N° 394. p. 93. *Tranſlated from the* Latin.

THO' a great many obſervations may be made both by day and by night on the latitude of *Lisbon*, yet F. *Carbone* had not hitherto an opportunity of diſcovering it; becauſe the inſtruments he had there, tho' ſufficiently adapted for finding degrees and minutes nearly, did not ſeem proper for finding minutes with certainty, much leſs ſeconds, which laſt ſhould by no means be overlooked by aſtronomers: And this the rather, becauſe there were different opinions about the Lat. of *Lisbon*, as to minutes; ſome of which, it is true, were nearer the truth, as appeared from F. *Carbone*'s own obſervations; yet he could not depend on any of them. He adduces only two opinions, each of which might be of conſiderable weight, were it not that they do not tally with each other: The firſt is that of *Emanuel Pimentel* Royal Coſmographer, and very well ſkilled in mathematics, who by ſeveral repeated obſervations, by means of the right ſhadow of a gnomon, 16 foot high, affirms, that he found the height of the pole at *Lisbon* 38° 48′ 20″, as F. *Carbone* had read in a M S. of this author, where he has laid down the obſervations themſelves at large: The other opinion is that of the *Royal Academy* at *Paris*, from the obſervations of

of F. *Couplet*, who came to *Lisbon* in 1697, where he made ſome obſervations to find the difference of meridians between that place and the *Royal Obſervatory*; as alſo to diſcover the Lat. of *Lisbon*; and he affirms he found it to be 38° 45′ 25″. In order to obtain ſome certainty in this matter, F. *Carbone* waited till he had got from *Paris* two very accurate aſtronomical quadrants, one of five and the other of three *Paris* feet, as alſo a ſextant of as many feet; and having accurately examined them ſeveral ways, he diſcovered no ſenſible error in them, but what might be very eaſily corrected, and is generally left to the care of the obſerver, ſuch as adjuſting the teleſcopic ſights. This he eaſily performed, and he began to uſe the ſaid inſtruments, in order to find the altitude of the pole.

He made ſeveral obſervations, ſome of which he here ſubjoins, on the ſun's altitudes, particularly, his meridian altitudes, taken either with the ſextant or aſtronomical quadrant of three feet; as alſo the computations, from which the altitude of the pole is deduced.

Some of the following obſervations were made in the *College* of St. *Antony the Great*, others in the Obſervatory in the king's palace; which places, ſince they are in the ſame meridian, if they differ at all, it is only in latitude; but the difference is not ſo conſiderable, as to be regarded in theſe obſervations; ſince he does not pretend to ſo great a degree of accuracy as to determine ſeconds.

		°	′	″
Nov. 24. 1725.	The altitude of the ſuperior limb of the ſun in the meridian by the aſtronomical quadrant	30	56	20
	The proper refraction of this altitude from Dr. *Halley*'s tables		1	28
	The correct altitude of this limb	30	54	52
	The parallax of the ſun			4
	The true altitude of the limb	30	54	56
	The apparent ſemidiameter of the ſun		16	18
	The true altitude of the ſun's centre	30	38	38
	The ſun's ſouthern declination	20	38	59

The

			°	'	"
		The altitude of the equator	51	17	37
		The complement or latitude of *Lisbon*	38	42	23
Dec.	5.	The meridian altitude of the sun's superior limb observed with the sextant	29	8	10
		The proper refraction of this altitude		1	35
		The correct. alt. of the same limb	29	6	35
		The sun's parallax			4
		The true alt. of the limb	29	6	39
		The sun's semidiameter		16	20
		The true alt. of the sun's centre	28	50	19
		The sun's southern declination	22	27	7
		The altitude of the equator	51	17	26
		The complement, or elevation of the pole	38	42	34
Dec.	6.	The alt. of the superior limb of the sun observed with the sextant	29	1	0
		The proper refraction of this alt.		1	36
		The correct. alt. of the same limb	28	59	24
		The sun's parallax			4
		The true alt. of the superior limb	28	59	28
		The sun's semidiameter		16	21
		The true alt. of the sun's centre	28	43	7
		The sun's S. Declin.	22	34	24
		The alt. of the equator	51	17	31
		The complement, or Lat. of *Lisbon*	38	42	29
Dec.	29.	The alt. of the sun's superior limb observed with the quadrant	28	20	22
		The proper refraction of this alt.		1	39

The

	°	'	"
The correct altitude of the sun's superior limb.	28	18	43
The sun's parallax			4
The true altitude of the sun's superior limb	28	18	47
The sun's semi-diameter		16	21
The true altitude of the sun's centre	28	2	26
The south declination	23	14	57
The altitude of the equator	51	17	23
The complement, or latitude of *Lisbon*	38	42	37
Jan. 8. 1726. The altitude of the sun's inferior limb observ'd with the sextant	28	47	10
The proper refraction of this altitude		1	37
The correct altitude of the sun's inferior limb	28	45	33
The sun's parallax			4
The true altitude of the sun's inferior limb	28	45	37
The sun's apparent semi-diameter		16	21
The true altitude of the sun's centre	29	1	58
The sun's south declination	22	15	42
The altitude of the equator	51	17	40
The complement, or latitude of *Lisbon*	38	42	20

Jan. 9. He observ'd two altitudes of the sun with the astronomical sextant before noon, and as many after, answering to each other in verticals, equidistant from the meridian; and for the greater accuracy, to the afternoon altitudes are added the corresponding scruples, which should be refunded from the sun's declination, however small, to the sun's vertical altitude.

altitude. These observations were made, in order to discover the least disagreement of the clock from true time, and at the same time examine the several meridian lines in the College: Both observations, compared together, did so exactly agree in giving the same difference, that their justness cannot be doubted of: He therefore thought these observations might also be properly used in finding the altitude of the pole, having these three *data*, *viz.* the sun's altitude, declination, and hour of the day.

	°	′	″
The sun's true altitude	20	36	18
His south declination	22	8	11
True time of the observ'd altitude	9^h	37	26
From these then by trigonometry results the elevation of the pole at *Lisbon*	38°	42′	24″
Again, from the second observation in the morning.			
The sun's true altitude	23	25	47
His south declination	22	8	10
True time of the observ'd altitude	10^h	4	41
From which, in like manner by trigonometry, results the height of the pole at *Lisbon*	38°	42′	25″

From the afternoon observations, in which almost every thing is the same as in the morning ones, the same height of the pole should be inferr'd, and therefore new calculations were unnecessary.

From the whole of the observations we may infer, that the Lat. of *Lisbon* observed at the College, or even, at the Royal Palace, does not exceed 38° 43′, nor is less than 38° 42′, but that it comes nearer to 38° 42′ 30″.

The disagreement of others in assigning this elevation, may possibly be owing, either to some defect in the instruments, made use of, (which could not well be the case in those of F. *Carbone*, being many in number and large, and wrought by different artists, and being often used, they always agreed) or to the different places, wherein the observations were made; for, *Lisbon* is pretty large, extending from south to north upwards of a league, which distance may cause the difference of three or four minutes: To which add the no small difficulty, which the most skilful usually experience in determining the extremity of the true shadow, and distinguishing it from the *penumbra*, and which M. *Pimentel* could not

 easily

easily have avoided in his observations by means of the right shadow of the gnomon: And therefore we are not to wonder, if some difference should happen, where the condition of the instruments is different.

An Occultation *of* Mars *by the* Moon; *as also Meridional Altitudes of* Venus, *both observ'd at* Toulon *in* 1725, 1726; *by* F. Laval. Phil. Transf. N° 394. p. 101. *Translated from the* Latin.

		°	′	″
1725				
March	20	36	34	40
Apr.	21	51	43	0
May	8	59	35	0
Sept.	8	44	30	30
	21	37	57	0
	24	36	26	30
Oct.	18	26	28	45
Nov.	8	21	50	0
Dec.	7	23	59	30
	21	28	21	0
	24	29	30	0
1726.				
Jan.	9	36	29	0
	19	41	19	0
	31	47	14	0
Feb.	3	48	40	30

Jan. 18. 1726 N. S. *Mars* was eclipsed by the moon at 7h. 23′ true time in the evening; but this not sufficiently certain.

He emerged at 8h. 21′ 34″ certain.

Observations on dissecting the Body of a Person troubled with the Stone; *by Dr.* Abraham Vater. Phil. Transf. N° 394. p. 102. *Translated from the* Latin.

A Studious young man, being troubled for two years with a frequent dysury, did in that time void upwards of 50 stones, most of which came away without any remarkable pain in making water; yet some of them, larger than the rest, and as big as large pease or french-beans, stuck in the *urethra*, and were either broken by the surgeon's hand, or extracted by incision. This patient gradually fell into a

marasmus

marasmus of his whole body; and at length afflicted with a dry cough and *asthma*, together with an oedematous swelling in his legs, he expired the second day after he took to his bed.

Upon opening the *thorax*, Dr. *Vater* found the lungs adhering on every side to the *pericardium*, diaphragm and ribs, and scirrhous, especially on the right side; besides considerable *polypus*'s in both ventricles of the heart, possessing the trunks of the vessels, and that, undoubtedly were the chief cause of the patient's *asthma*, subsequent suffocation, and sudden death.

In the *abdomen* the liver and spleen had no visible blemish; but the *ileon* was discoloured, and the *colon* in its whole circumference from the right side, where it lies upon the liver, as also the *rectum* appeared contracted in such a manner, as hardly to be as thick as one's finger, and without any internal cavity at all.

At length upon viewing the urinary passages, as being the seat of so many *calculi*, he discovered nothing preternatural in the kidneys and *ureters*; but in the bladder he observ'd three stones, as big as *French* beans, not loose, but involv'd in a thick membrane, and adhering to the fore-part near the sphincter. It seemed very difficult to Dr. *Vater* to explain whence this membrane, that involv'd the *calculi*, took its rise; wherefore, having, on this occasion, inflated both the *ureters*, he wanted to know whether the said membrane communicated with them; or whether it were the internal membrane of the bladder; but he could discover nothing to this purpose: However it seems consonant to reason, that these *calculi* did not only occasion a continual dysury, but likewise, on account of the constant irritation, the preternatural constriction of the *colon* and *rectum*.

Observations on the Dissection of a Male Ostrich; *by Mr.* George Warren. Phil. Trans. N° 394. p. 113.

DR. *Brown* has (*Phil. Collect.* N° 5.) so well describ'd the parts of the ostrich he dissected, that Mr. *Warren* thinks there is not much to be added. But the Dr. affirms it had no *epiglottis*; whereas, in this subject, that cartilage was plainly visible, and indeed, the *rimula* appear'd too open, not to require one. The *os hyoides* is three inches long from the basis, the *musculi directores asperæ arteriæ* were very plain, large and strong; the ring, compos'd of three

cartilages at the divarication of the *aspera arteria*, was very strong; the two glands on the carotid arteries were of the size of small eggs. There was nothing in the lungs or heart, but what it has in common with other birds. The two stomachs, namely the crop and gizzard, were fill'd with half digested grass, in which were some nails, some stones of the bigness of walnuts, and about 14 or 15 pieces of silver, and copper coin. The first stomach, or crop was exceeding tender, and contain'd, cramm'd as it was, between three and four quarts. The glands on the top of the crop were very large, and numerous, in the order describ'd by Dr. *Brown*, and as big as the small eyes of sea-crabs, and of a watery brown colour; which, being so different from the colour of the stomach, and added to the pretty order they are placed in, makes them very remarkable. The crop lay within the *thorax*, but so as that the gizzard lay higher. The looseness and resemblance of flannel of the inner coat of the gizzard, as mentioned by Dr. *Brown*, was very remarkable in this subject; but the texture of the muscular part thereof did not seem proportionably strong to that in other birds, being broader, thinner and more flaccid. The guts, as near as Mr. *Warren* could measure them, were about 26 yards long. The two *cæcum*'s, which are about 34 inches long each, and have beautiful spiral valves, were appendages of the very beginning of the *colon*. The *testes* lay as in other birds, very high, and less than pidgeon's eggs, but longer. He found the liver to have four lobes, and thought he observ'd a gall-bladder, but it appeared at last to be only the membrane of the liver, rais'd by some accident from its inner substance. The gland under the stomach, which Dr. *Brown* supposes to be the spleen, as also the *pancreas* and kidneys, answer his description; and the *ureters* were, as he says, firm, strong, white, long, and opening into the *rectum*. The eye is said to be exactly like the human eye; but it is a perfect goose eye as to its colour; and Mr. *Warren* believes, as to the rest of its parts, that it is, as they are well describ'd by Mr. *Ranby*: It was flatter than the human eye, as it is, Mr. *Warren* believes, in all birds; and it had that simple look so peculiar to the goose. This bird has in common with other fowls, both of the land and water, the bony circle describ'd by Mr. *Ranby*, only with this difference, that the ring in water-fowls consists of 15, and in land-fowls but of 14 bones. They are dispos'd in such a manner, that one bone lies over the ends

ends of two others; then three or four bones lie over one another, like the ſcales of fiſh, then one bone lies under the ends of two others; and then two or three more follow again, like the ſcales of fiſh; but unleſs there be a *luſus naturæ*, Mr. *Warren* thinks Mr. *Ranby*'s figure does not repreſent it ſo very juſtly as Mr. *Warren* thinks it might be done. There was no *muſculus ſuſpenſorius oculi* in this animal; nor does Mr. *Warren* believe it is to be found among birds, and indeed there ſeems to be no reaſon for it.

The crop was ſo ſtuff'd with graſs, or rather greens, (proper food for a gooſe, or one of that kind) that Mr. *Warren* does not think the bird could have digeſted it all, if there had been no other reaſon for its death. The gizzard was not ſo ſtuff'd, as the crop, and what was contain'd therein ſeem'd undigeſted. The guts contain'd a thick deep green juice, even to the *cloaca*. The pieces both of ſilver and copper coin, in the gizzard, were very remarkably worn away; particularly, the edges were made round, and the buſt and reverſe ſcarce perceptible in ſome pieces, and quite obliterated in others. The *erugo* and *ſulci* in ſeveral of the pieces would make one apt to think, that, beſides the attrition, there may be a *menſtruum* in their gizzards, not unfit to diſſolve metals. Within an inch of the end of the *rectum* was the *cloaca* or expanſion of that great gut, which was thinner than the other part of the gut, in proportion to its expanſion, and would hold above half a pint. The end of the *rectum* (from the *cloaca*) opened into a cavity big enough to hold his two fiſts; and for want of another name, he calls it *receptaculum penis*; becauſe the *penis*, when flaccid, was always lodged therein. That part is call'd by Dr. *Brown* a kind of prepuce; but upon diſſection, it appeared plain enough to Mr. *Warren* to be a very ſtrong muſcle, compos'd of circular fibres, and to be deſign'd for a ſphincter of that part in which the *penis* was to be lodged, as alſo for a ſphincter of the *rectum*, round which he traced the ſame muſcle upwards of an inch; and this being but one muſcle muſt be the reaſon, that the *penis* came out ſome inches when it muted, as he was told it did. The *penis*, flaccid as it was, was five inches and a half long from the ſkin of that *receptaculum*; and as Dr. *Harvey* ſays not unlike a hart's tongue. Mr. *Warren* did not find a cartilage in it, as Dr. *Brown* ſuggeſts; but at its origination it is

so hard, that, he believes, if the bird had lived some years, it possibly might have become cartilaginous. There are two bodies joined to the *crura penis*, which he suspected to be the *vesiculæ seminales*; and the rather, because there are two vessels that enter them, which he takes to be the *vasa deferentia*; but he is not assured of this: For, tho' he found *semen* in the *urethra*, he could not trace a passage from these supposed *vesiculæ seminales* or those vessels, or any other part into the *urethra*: Mr. *Warren* calls it *urethra*, because there is no other term fixed that he knows of, tho' the urine does not pass that way; but as in other birds, is mixed with the grosser excrements in the *cloaca*. The *urethra* then is only a *sulcus* or gutter from one end of the *penis* to the other; which *sulcus* as the *penis* lies flaccid in the *receptaculum*, lies on one side; but upon erection, the *penis* turns towards the belly, and the *sulcus* is then at the top, and lies conveniently enough for conveying the *semen*. If those two bodies are not the *vesiculæ seminales*, they must be elongations of the *crura penis*; but he thinks they are of much too loose a contexture to serve that purpose. Whether the *vena cava*, dividing into two branches to go into the kidneys, and uniting again when it comes out, be peculiar to this bird, or common to it with geese and other water-fowl, Mr. *Warren* could not determine; but so it was in the ostrich. But he supposes it to be in common, till he has examined farther; since he knows, that the *cæcum*'s of the ostrich, which are so much taken notice of, are no more than what it has in common with other fowls; and that a chicken has two as large and as long in proportion, as the ostrich: Mr. *Warren* had too little time, and the ostrich too much fat to make a more accurate dissection. The *omentum* upon the stomachs and guts was six inches thick at the top, and gradually decreasing, was near two inches thick in the vent, and divided into two parts in the middle from top to bottom. What he found was common to it with other fowls, he has not taken much notice of, unless represented by others differently from what he found, or peculiar to this bird.

The round part of the top of the *os hyoides* is lodged in a proper cavity in the top of the tongue. Partly under the basis of the *os hyoides* there lies a cartilage in the fore part, and very beginning of the *aspera arteria*, which is not unlike the *thyroides*; but it has no other cartilages in that part, but what form the *rimula*. The first 28 cartilages of the *aspera arteria* are

are not annular, the reſt (being about 226) are entirely annular; but as ſoon as it divaricates to go into the lungs, they are not ſo.

Effects of Lightening *at* Worceſter, June 11. 1724; *by Dr.* Beard. Phil. Tranſ. N° 394. p. 118.

WE had on the 10. of *June* continued lightning in the eaſt from 8 o'clock at night to 12; the weather having for ſome time before been very ſultry, the wind at N. E. and the barometer at ſettled fair. Next morning the mercury ſunk, and the ſky became more cloudy and temperate, excepting a few hot gleams; at two o'clock in the afternoon, ſeveral fierce ſhowers fell, attended with flaſhes of lightning, and claps of thunder, that ſtill approached nearer us: Between 2 and 3 o'clock, a flaſh came ſo violently upon Dr. *Beard* and was ſucceeded ſo very quick by a low, unuſual dreadful ſound, that he immediately went to the door, apprehending ſome miſchief near; and ſoon after he was called to an officer's lady (aged about 18 and breeding) killed by it in the adjoining ſtreet. He found her ſtill warm, and that ſhe had ſurvived the ſtroke for ſix or ſeven minutes. The fire-marks were ſtreaks of a copper-colour, branched from the left ſhoulder all over the *thorax*, and interſperſed here and there with irregular ſpots, which gave occaſion to that conceit, publiſhed in our news, *that curious plants were drawn on her boſom, as with the fineſt pencil*. This accident happened in a parlour window next the ſtreet, that could contain about two perſons. The Lady, it ſeems, frightened with the repeated thunder and lightening (it having formerly been fatal to her brother) deſired an officer to change places with her, that ſhe might be near her husband; but ſhe was no ſooner ſeated by his ſide, than ſhe inclined ſideways, and ſpoke ſome words; after ſhe was carried to another room, ſhe ſaid, ſhe was gone, and then that ſhe was blind, and aſked for water. The husband was thrown along, together with the Gentleman that had juſt reſigned his ſeat; and a large looking-glaſs was lifted off the hooks. The landlord's daughter, at work near the Lady, perceived ſuch an impulſe on the ſide of her head, that her hearing was much impaired; and upon every peal of thunder after, ſhe is affected in like manner, tho' not ſo ſtrongly. The Gentlemen complained that they were ſtupified, and forced down for no other reaſon they knew of, unleſs it were for want of breath; as alſo of pains and numbneſs in their limbs; they had likewiſe on different parts of

their

their bodies ſuch reddiſh wheals, as were obſerved on the Lady's breaſt; but theſe ſymptoms vaniſhed next day. The other two perſons at the farther end of the room were untouched; but they were all ſenſible of a ſulphureous ſtench. The pane of glaſs, exactly behind the Lady's waiſt, was perforated with a round hole an inch and a half in diameter, as if done with a diamond, or rather a wind-gun; but no where thereabouts could the Dr. diſcern the leaſt traces of fire, or heat; nor on the Lady's cloaths (having no ſtays on) the ſigns of any violence. On a more nice examination of the body that evening, he diſcovered on the left loin, taking in part of the ſpine of the *os ilium* (which was ſomewhat ſwoln) a deep contuſion of the ſame dimenſion with the breach in the glaſs: The ſkin was neither indurated, nor pierced: The blood was ſettled in the capillaries quite round; but chiefly up the back; the colour of which was eaſily diſtinguiſhed from that of the ſtreaks, and the circular impreſſion.

The phenomenon, that cauſed this misfortune, roſe from the N. E. firſt ſlid off the gabel-beam, and the bricks on the back-part of the next houſe, filled a little court with flame and ſmoke; then turned a leaden ſpout contrary to its former direction, mounted over the roof, and cracking a ſtack of chimneys, dropt down at the window, where the huſband and wife ſate.

Some credible people that ſaw it (to their great terror) aſſured the Dr. that it was a ball of fire; and that it burſt with the loudeſt report they ever heard; and then with a hiſſing noiſe paſſed about a yard from the ground through an adjacent ſtreet, and rolled off to the S. W. Some workmen there and on a neighbouring hill, obſerved the ſame.

The Dr. thinks, the mortality of this blow may be accounted for from the known effects of impriſoned air only, when ſet at liberty; as the appearances on the ſkin may from other active particles hurried along with it at the time of the exploſion. The *impetus* being firſt received on the parts deſcribed, occaſioned the Lady's death to be leſs ſudden than is uſual in ſuch caſes.

We have had more thunder and lightning in one week, than ever has been known in that ſpace of time: And what was more extraordinary, was the continuance of it for 9 or 10 hours together, with little or no intermiſſion, and its being at ſuch a height above us. So far has this been from doing any great damage near us, that in the opinion of the country farmers, it had very good effects, eſpecially, at the beginning during the heat:

heat: For the little insects, that in some places threatened the destruction of the hops and some other plants, fell off like bees by the steam of a lighted match.

An Account of the Strata *met with in digging for* Marle, *and of* Horns *found under Ground in* Ireland; *by Mr.* James Kelly. Phil. Transf. N° 394. p. 122.

OUR marle is found no where but in the bottoms of low bogs, where we search for it with augres, and find it at the depth of 7, 8, or 9 foot: This in several places occasions great expence in draining off the water. When we intend to dig for it, we chuse out six able labourers, and a supernumerary; then we cut up a hole 12 foot square; because we judge that this number of men will manage that pit in one day, *viz.* two men to dig, two to throw it up, and two to throw it by. The supernumerary supplies defects in every part, as shall be found necessary. For the first three foot we meet with a fuzzy sort of earth, called *moss*, proper to make turf for fuel; then we find a *stratum* of gravel about half a foot; under which for about three foot more, we find a more kindly moss, that would make a more excellent fuel: This is altogether mixed with timber, but so rotten, that the spade cuts it as easily, as it does the earth: Under this for the depth of three inches, we find leaves, for the most part oaken, that appear fair to the eye, but will not bear the touch: This *stratum* we find sometimes interrupted with heaps of seed, that seem to be broom or furze-seed; nay, in one place Mr. *Kelly* observed what appeared to him to be goose-berries and currans: In other places in the same *stratum* we find sea-weed, and other things as odd, at such a depth: Under this appears a *stratum* of blue clay, half a foot thick, entirely mixed with shells: This we look upon to be good marle, and throw it up as such: Then appears the right marle, commonly two, three, or four foot deep, and in some places much deeper, which looks like buried lime, or the lime that tanners throw out of their lime-pits, only that it is entirely mixed with shells: These are small periwinkles, such as in *Scotland* they call *fresh water-wilks*; tho' there are among them abundance of round red periwingles, such as Mr. *Kelly* has often observed thrown out on the sea-shore. Among this marle, and often at the bottom of it, we find very large horns, which, for want of another name, is called elk-horns: Where they are joined to the head, they are thick and round; and at that joining there grows out a branch of about a foot long, that

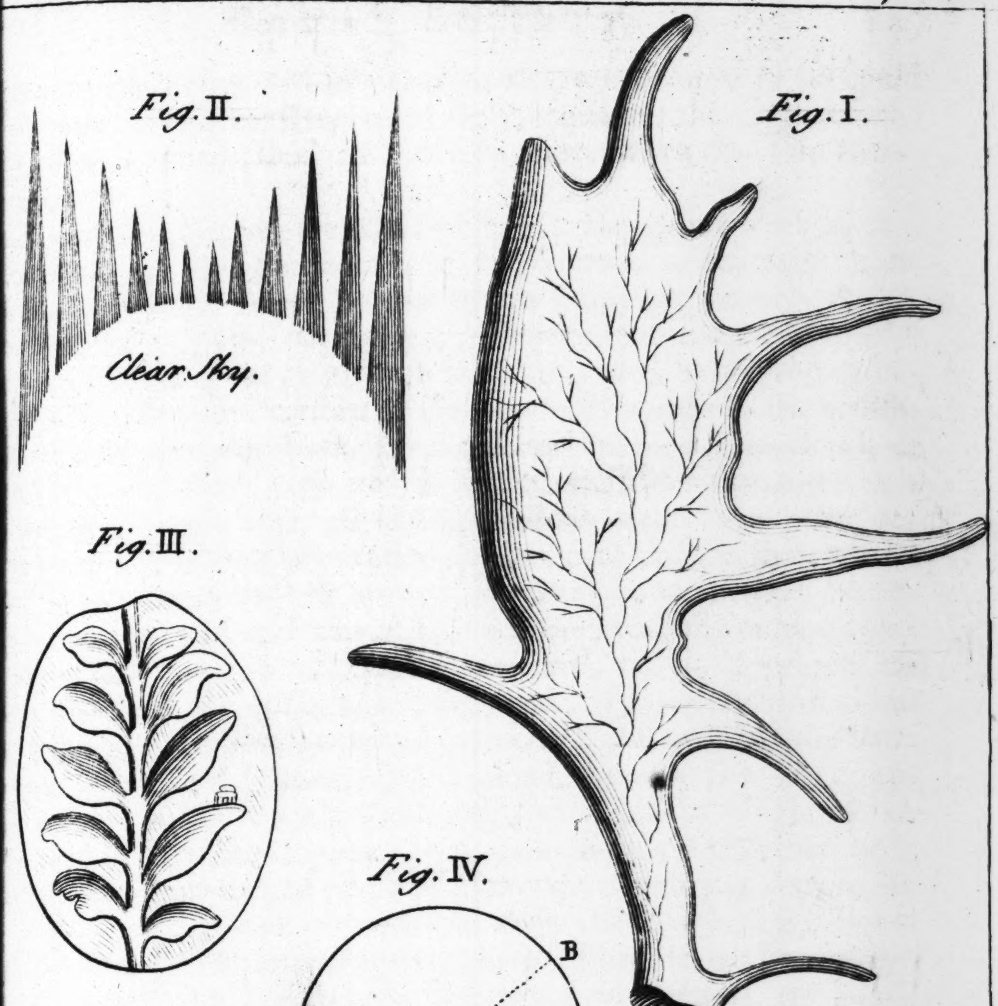
ſeems to have hung juſt over the animal's eyes: It grows round above this for about a foot and upwards; then it ſpreads broad, and ends in branches, long, and round, turning with a ſmall bend. The labourers are commonly ſo hurried, that they rarely bring them up whole; however Mr. *Kelly* had one that was pretty well, as repreſented Fig. 1. Plate IV. tho' not ſo nicely drawn as he could wiſh. We have alſo found ſhanks and other bones of theſe animals in this place.

An Account of an Aurora Borealis *in* Ireland Sept. 24, 25, 26, 1725, *with a ſolution of the* Phenomenon; *by Mr.* Arthur Dobbs. Phil. Tranſ. N° 395. p. 128.

MR. *Dobbs* obſerved, that the ſcene of light, forming an irregular variable curve, was, as at moſt times before, from E. N. E. to W. N. W. the horizon and whole hemiſphere ſerene, little or no wind, and what there was ſeemed northerly. The ſeeming dawn or ſcene of light generally continued in an irregular curve; the one point for the two firſt nights whilſt he obſerved it, began near the horizon, near N. N. E. the other point was at W. N. W. the height of the arch not exceeding 20 degrees, in which there ſeemed to be a continual dawn: Under that field of light there ſeemed to be a dark cloud, which, however, was a clear ſky, not filled with that luminous vapour; becauſe all the ſtars appeared diſtinctly and twinkling thro' it. Whenever that light roſe about 10 degrees higher, *viz.* to about 30 degrees; then flaſhes, or coruſcations followed alternately, and ſeemed to be columns, or beams of light, which followed or ſucceeded each other, and by that means ſeemed to move and change with one another, by the ſucceſſion of light and darkneſs, according to the flaſhes. When the lighted vapour roſe higher, as to about 40 or 45 degrees, then the appearance altered; and inſtead of beams or columns of light, as when lower, there were flaſhes like thoſe attending exploſions, wherein faint colours of red, green, and yellow appeared, but not very vivid; and upon each exploſion it would ſpread upwards towards the zenith, like thin enlightened clouds, and immediately diſappear. On the 26. of *September* about 9 o'clock at night, one of theſe irregular arches of light had got up to the zenith, the lower points being near E. N. E. and W. S. W. He then obſerved it for a conſiderable time, at leaſt a quarter of an hour; and it had been there for ſome time before he ſaw it. He could diſtinctly obſerve all the different appearances, according to its altitude in the hemiſphere, *viz.* the

lower

PLATE. IV.



J. Smith del.

Hulett Sculp.

lower part (being within 12 or 14 degrees, as near as he could compute) was a constant fixed light, equal to the light of the edge of a white cloud in the day time, when the sun shines on it.

As it rose higher, he could observe it somewhat weaker, and he could perceive the motion of the columns or beams of light after each flash, which by that means seemed to move. Somewhat higher again, at about 40 degrees, the flashes were like explosions of great guns, with the faint colours observed as before; but the coruscations or flashes from thence to the zenith, expanded at every flash, like a broad, thin, white cloud, of which some faint view could be seen for some time after each explosion: And after all the explosions were over, there remained a thin duskish vapour in and near the zenith, and all along the arch from east to west, from 14 to 20 degrees broad, which undulated and moved like a stormy sea, the motion coming from the S. S. E. and so lessened, till it appeared no brighter than the *milky way*; but more like a very thin cloud or mist, thro' which he could perceive the stars. At the same time he observed another thin cloud, having the same appearance, archwise to the southward, at about the height of 40 degrees, which he supposes had been another, that had been over, and had moved thither from the northward before he went out: And during the whole time there were lesser lights towards the north, but dispersed here and there, and not forming any large body of light. During the whole time, the hemisphere was clear, excepting a few very small clouds near the horizon; and when any moved into the enlightened arch, they broke the connexion; so that the light was above them: At the same time it froze hard each night.

From these observations, Mr. *Dobbs* supposes, that the *aurora borealis* is a thin nitro-sulphureous vapour, raised in our atmosphere considerably higher than the clouds, which is discontinued in several places by the interspersed air, and which is kindled by pressure and motion; and perhaps, the explosion of one may by its shock and motion contribute to kindle the next; by which means they go off one after another, till the whole vapour within their reach be discharged; and then the light disappears, and the thin smoke appears, and undulates according to the motion in that part of the atmosphere: And hence he thinks most of the appearances may be solved. For, 1. as to the continued light near the horizon, these apperances being at a great distance from us, and nearly in a line, all these explo-

T 2 sions

ſions may ſeem as one continued light; when theſe approach nearer to us, and conſequently appear higher in our hemiſphere, we obſerve the motion in each flaſh, and ſtill ſeeing them laterally, yet ſomewhat breaking the continuity of the light, they (by the reflection of the vapour floating in the atmoſphere, and not being reflected, where the air between them is free of thoſe vapours) may appear as columns; and as the flaſh below and beyond them moves (as it kindles and expands) ſo they ſeem to move, and probably, ſhock at the ſame time by the motion; but afterwards when they are nearer, and raiſed to the altitude of 40 degrees, we get ſomewhat under them, and ſee the expanſion of the exploſion, which appearing ſomewhat globular, gives the faint colours obſerved above, the light not being intenſe enough to render them vivid; and afterwards, when they riſe ſo, or near the zenith, they are higheſt; and then expand very wide at each flaſh, like little clouds: And Mr. *Dobbs* thinks the great objection of their appearing in the northern part of the hemiſphere, and ſeldom or never in the ſouthern, is in ſome meaſure anſwered by the appearance on the 26. ſince at leaſt half of the arch was in the ſouthern part of the hemiſphere; and perhaps the reaſon why the light is not ſeen near the horizon, in the ſouthern part of the hemiſphere, may be this, that in clear ſerene weather, the wind being generally near the north, objects from thence are much more diſtinctly viewed, and at a greater diſtance than from the ſouth; and it is generally known, that lands at a great diſtance are moſt diſtinctly ſeen, when the winds blow from them.

And probably, a cold, northerly freezing air may be neceſſary to kindle the vapours, when a contrary motion above (namely, higher in the atmoſphere) may carry the ſulphureous vapour, which, falling down from the nitrous vapour, may be kindled: Which, he ſuppoſes, form the undulations of the ſmoke after the exploſion, which ſeemed, as above, like a ſtormy ſea moving from the S. S. E.

Note, the barometer was low for ſome days both before, and after it.

An Account of an Aurora Borealis *at* Petworth *in* Suſſex *October* 8. 1726, *with Obſervations thereon; by Dr.* Langwith. Phil. Tranſ. N° 395. p. 132.

THESE lights began about ſun-ſet; but Dr. *Langwith* heard nothing of them till between 7 and 8. When he went out, he obſerved a ſtream of light almoſt due W. which

which was about 7 or 8 degrees broad, and extended itſelf upwards of about 35 or 40 degrees. He had not a free proſpect of the weſtern horizon; and ſo could not tell what its appearance was below. It was not perpendicular to the horizon, but inclined a few degrees towards the ſouth. This ſtream was of a duſky red towards the north, but pale on the other ſide, and ſeemed to have a faint mixture of the priſmatic colours in it.

At the ſame time there appeared a pale luminous arch, whoſe middle was nearly N. W. by N. The altitude of its inner edge was about 18 or 20 degrees. This edge was very diſtinct, and regular all above, but a little confuſed towards the horizon, where it extended itſelf beyond the north-point: How it terminated to the weſt, he could not tell. From the upper ſide of this arch, which was waving, and ill defined, there ſhot up continually ſuch ſtreams of light, as have often been obſerved and deſcribed, ſince the great meteor of *March 6. 1715-6.* The ſky under this arch looked exceeding dark, but was really clear; for, we could obſerve the ſmalleſt ſtars in it.

Nearly N. E. there was another ſtream of pale-coloured light, which was about 7 or 8 degrees diſtant from the horizon, and about as many in breadth; its height was various and ill-defined; towards the bottom of it, was an irregular black cloud, which in ſome parts was near a degree in breadth, in others hardly half ſo much: This cloud was almoſt parallel to the horizon. The ſtream moved with a ſlower regular motion towards the eaſt.

In the S. E. was another arch, like that in the N. W. by N. but not quite ſo high, or of ſo great an extent.

Between this arch and the north eaſterly ſtream, the ſky was of an odd pale coloured light, with a mixture of red in it.

From the ſouth towards the weſt there were gloomy irregular clouds, which now and then emitted flaſhes of light.

About 8. the north eaſterly ſtream ſuddenly expanded itſelf every way: All its parts began to be in a violent commotion; and its brightneſs increaſed to ſuch a degree, that he remembers nothing like it in the former great meteor of this kind. All above it was of a bright flame-colour; but below, it was edged with the priſmatic colours, which were full as ſtrong, as he ever obſerved them in the brighteſt rainbow; they were not, it is true, ſo diſtinct: For, tho' he obſerved them as exactly as the ſtrange variety of their motion would permit, he could only diſtinguiſh the red, the yellow, and a duſky blueiſh green.

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This surprising sight did not last above a minute or two; but when the colours vanish'd here, they began to appear in the north westerly arch, which was now become a portion of a larger circle than before, and was not elevated so high above the horizon. The colours extended themselves from the north towards the west for about 15 or 20 degrees; and tho' they were not so bright as in the other place, yet they were more steady, and so more easily observed. Their order was the same as before, the red lowermost, and so on: Their duration was much longer.

In the mean time the streaming lights began to appear in all parts of the heavens, and to form a *corona* and canopy, which in all respects were like those of the great meteor of 1715-6, of which *vide* the curious descriptions given by Dr. *Halley* and Mr. *Cotes* in a former *Transaction*. Dr. *Langwith* only takes notice, that the colours of the *corona* were neither so strong, nor so lasting, as those before describ'd, and that the top of the canopy was something overspread with a deep sullen red.

The streams continued their direction upwards, towards a point of concourse for a long time after, and form'd by its imperfect circles of pale light about it: This point, however, was not fix'd; for, at first it seemed to be in, or very near the zenith; but when he observ'd it some time after, it lay between the stars in *Andromeda*'s right hand, and those at the end of her chain. The same observation was made by a curious gentleman of *Petworth*, who also informed the Dr. that there was another luminous arch, which pass'd quite thro the pole star: Its continuance was short, and the Dr. had not the good fortune to observe it himself.

These appearances held on in some measure till about 11, when the air began to grow misty, and so put an end to any farther observations.

It was so calm below, that the Dr. could not be certain which way the wind stood; but some, that were making their observations from a high open place, assur'd him, that it was north westerly, as it was in the afternoon before, and the morning after.

The mercury was up at 30; the weather mild and temperate.

The Dr. farther adds the following observations;

1. That it plainly appears from the position of the arches, that they could not owe their figures to the sun; they seem to have

have been partly optical, and partly to have depended on the different heights of the luminous vapours; but for want of sufficient *data*, it will be no easy matter to determine, how far each of these causes concurr'd.

2. The prismatic colours, wherever they appear'd, seem to have been caus'd by the sun.

3. None of the streams, so far as he could observe, proceeded directly from the horizon. They were nearest it towards the north, where there were some weak irregular lights in the confus'd parts of the arch before describ'd.

4. The Dr. finds by some of his papers, that during the meteor of 1715-6, the mercury stood at 30.2; so that the two meteors agree, as in several other particulars, so in the following, *viz.* that the air was calm, the wind north westerly, and the mercury high.

He only adds farther, that luminous vapours in the air are much more common than they are generally taken to be; for, the nights are very often lighter, when the sky is overcast, than in the brightest star-light, tho' the *crepusculum* be quite gone off, and there be no moon.

An Account of the same Phenomenon *at* Plymouth; *by Dr.* Huxham. Phil. Transf. N° 395. p. 137.

ABOUT half an hour after six o'clock in the evening, *October* 8. Dr. *Huxham*, perceiving *Jupiter* shone very bright, was applying his telescope to observe him; when on a sudden several luminous streaks appeared about ten degrees above the horizon in the N. E. and the hemisphere seemed much enlightened. Imagining this to be the beginning of a *lumen boreale*, he carefully cast his eye along the northern horizon from E. to W. and very nearly in the W. point he perceiv'd a vast red fiery colour'd obelisk, as it were, shoot itself up to the height of 30 or 40 degrees, which seem'd perpendicular to the horizon, and its base to insist on it. Its point almost touch'd the bright star in the northern crown; a smaller column or two stood nearer, of the same colour and shape. In the mean time, the light increas'd considerably to the eastward, and became more vivid; as when the moon is behind a very bright cloud: It also form'd itself into columns, which were projected to no great height, and would soon vanish, then soon return; and appear'd not only in the N. E. but also more northerly.

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In about a quarter of an hour from his first observation, as from an arch or black basis, as it were, extended all over the northern horizon (which seem'd to intersect it nearly in the W. and E. N. E. points) arose abundance of pyramidal columns of light on all parts of it; now here and now there, of unequal bigness, height and lustre; now suddenly gleaming forth, then as suddenly disappearing; but those columns to the eastward of the N. were more bright and lucid than those to the westward, which were of a more fiery rutilant colour. The great column in the W. still remain'd in the same position, height and shape; as he observ'd, by applying his eye to a wall very near E. and W.

Between the arch and the horizon, appear'd a black, dusky fog, as it were, from whence the streams of light seemed every where to dart forth: Yet however black this appeared, we could discern the stars very clearly thro' it. This arch, at its first appearance, seemed not to be above 15 or 20 degrees (at its highest part) above the horizon; but it continually grew higher; and from all parts of it cones of light were every moment shot up, which all seem'd to tend to a point near the zenith (as the vertical circles or arches on a globe tend to its poles) tho' hitherto none had reach'd it by several degrees.

After seven o'clock the columns to the westward appeared bright and vivid, as those in the E. excepting those very near the W. Tho' the limb of the arch would seem sometimes very regular, and well defin'd; yet at other times it would seem to sink, now in the middle, then at one part, and then again at another; and sometimes it would rise with the same irregularity: But it was certain, that during the whole time of the phenomenon, no light, nor flashes of light appeared in the black area, included between the arch and the horizon; even when it was at its greatest height, which was about 10 or 12 minutes before eight o'clock when he judged it to be at least 40 degrees above the horizon: Then from all parts of the arch; but first from the northern or highest parts of it, were rays, or lucid columns of a surprising brightness and lustre, darted with incredible velocity towards the vertex, where the cusps of the converging columns seem'd nearly to center; and suddenly from every quarter of the heavens, bright, shining streams of light were shot towards the zenith; which meeting about six or eight degrees to the southward of it, form'd a small circle of two or three degrees in diameter,

whose

whose border was much more lucid than near its center: This circle seemed to be form'd between the *Swan's tail* and the *Lizard*, then nearly upon the meridian.

This beautiful *spectrum* might be likened to the star worn by the most Noble Order of the Garter, but the pyramidal *radii* were here revers'd; and the rays or *striæ*, from the southward were not near so long as those from the N. especially those from due S. not reaching above 10 or 15 degrees from the center or circle: Whereas those from the eastern and western quarters were very long, and reach'd almost down to the horizon; especially, in the E. and W. points. The *radii* were in a continual and exceedingly swift undulation, and appear'd of several very bright colours, as white, red, green, yellow, for several seconds; but the most permanent and predominant colours were a fiery red, with an eye of crimson, and a bright pearl colour: The red rays came mostly from the westward, and that colour continued till the entire dissipation of this radiant canopy; the others dying away, and leaving a thin smoke, as it were. The vibrations of these radiant columns were incessant, and as swift as flashes of lightning,

This surprising sight remain'd over us in its full glory three or four minutes; during which time the rays were darted towards the center with prodigious swiftness, and did not seem to be shot from it. Sometimes they undulated like the vapours arising from a lime-kiln, or from the earth in very hot weather, and all the upper part of the hemisphere seem'd to be in a convulsion, as it were.

In a short time this agreeable scene vanish'd, and was broken into small flitting bright clouds, which still retain'd an undulating motion; and coruscations would every now and then break out from them. At this time he observ'd several star-like meteors fall, as is frequently observ'd in a bright serene night.

Tho' our glorious *cupola* disappear'd a very few minutes after eight; yet very vivid coruscations were continually shot from the N. E. and N. W. parts of the heavens, which dashing against one another near the zenith, form'd by their collision momentary arches of a circle, nearly in the same place and of the same diameter with that above-mentioned. None now proceeded from the S. and very rarely from the true N. The coruscations were always more red and fiery from the west than

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from the east, which were always more bright and luminous.

We were lost in the contemplation of the beautiful phenomenon over head, and did not observe the formation of a lucid arch projected over all the northern horizon, which seemed like the arch of a rainbow, of one vivid, bright, yellowish colour; and all under was a very dark cloud, as it were; tho' upon viewing it with a telescope, we could discern the minutest stars: So that the darkness only proceeded from the greatness of the light just over it. From this, as from the former, arose very lucid, bright columns on all parts of it: No coruscations appear'd under it. Its greatest height might be 20 or 30 degrees: Some of the columns seemed to radiate from this arch, even to the zenith.

About 9 o'clock this lucid arch vanish'd insensibly, with most of the luminous *radii*, or columns; but a very bright *crepusculum*, as it were, still remain'd all along the northern horizon, and several very bright coruscations would seem to be shot out of the pure sky: This was more especially observ'd in the N. E. About 11, the Dr. observ'd several coruscations still breaking forth, and here and there a luminous column; and several small bright clouds seemed irregularly scatter'd up and down the hemisphere, that still retain'd their darting and undulating motion. The northern *crepusculum* remain'd as bright as ever, and continued so till past two in the morning.

There were but very few, and those very small clouds to be observ'd, during the whole time of this phenomenon, and the air was clear; yet all around, and between the lucid columns, whenever, or in what part soever, they appeared, the air would seem very thick and hazy; tho' immediately upon the disappearing of those gleaming lights, the sky would in the same place appear very clear and serene: Nay, even thro' some of the very columns one could plainly discern the stars. Some gentlemen thought they saw the bright stars of the *Swan* thro' the *corona* itself.

As to the weather preceeding and following this phenomenon; the morning was fair, tho' the air was thick, and there was a great dew; the mercury was at 30 inches; *Hauksbee*'s thermometer at 50; little or no wind; the day was pleasant and warm; and the air became much thinner: The evening was serene; a very soft breeze from N. and by W. About five next morning, there were several clouds form'd, and the air was

very

very thick and hazy; at ſeven it was all cloudy, and a few drops fell.

Tho' the Dr. had before obſerv'd ſeveral faint appearances of the *aurora borealis*; yet this for beauty, luſtre and duration, vaſtly exceeded any thing of that nature he had ever ſeen. It is true, he did not ſee that of *March* 6. 1715-6, not being then in *England*.

The ſame Phenomenon *obſerv'd at* Exeter, *together with an Account of its Cauſe*; *by Dr.* Hallet. Phil. Tranſ. N° 395. p. 143.

OCT. 3. 1726, at 9 o'clock in the evening, Dr. *Hallet* obſerv'd an *aurora borealis*, as it is commonly call'd, in which there was nothing different from former appearances, only that from the luminous arch, which appeared in the north, parts of arches were frequently ſhot off towards the zenith, and vaniſh'd there.

Oct. 8. Coming from the country near 7 o'clock in the evening, he obſerv'd a great light in the eaſt and weſt, which ſoon extended itſelf over head, the north and ſouth appearing dark at the ſame time: No cloud was ſeen all that day. A great dew fell all on a ſudden, with which the ſtreets were wet, as by a ſmall rain. Half an hour after ſeven, many ſtreams appear'd in the north, which grew very bright, and frequently darted up to the zenith. A line drawn thro' their baſes made an arch of a circle, extending from the N. E. to the W. or S. W. But the ſtreams ſeemed to proceed from a clear ſky, being diſtinct from one another at the baſes, and not united by a luminous arch or cloud, as in the more common *aurora*'s. The ſtreams, at the two extremities of the arch, were brighter, wider, and longer when they did not ſhoot, than thoſe on the top of it, Fig. 1. Plate IV. There was at the ſame time a luminous arch extending itſelf from the two extremities of the above-mentioned *aurora* thro' the ſouth, at a conſiderable altitude: About eight o'clock the ſtreams began to have an horizontal motion, propagating themſelves on both ſides towards the ſouth; and in a minute or two the whole heavens were ſurrounded with them: Immediately they all extended themſelves up to a point near the zenith (the Dr. thinks, a little towards the eaſt) where their points were blended together in a confus'd manner. At the ſame time every ſtream, which before was white, appeared ſtriped with all the colours of the rainbow; but the moſt pre-

vailing colour was a deep red. It is impoſſible to repreſent the beauty of this glorious *umbrella*, which cover'd the whole hemiſphere with its variegated rays, the colours of which ſucceeded one another in a regular order. In the center of theſe rays was a confus'd rolling, agitation or ebullition of a luminous cloud, reſembling ſmoke. In about 10 minutes (as he ſuppoſes) the colours diſappear'd, and the ſtreams began to retire from the zenith; preſently after which, they would frequently dart and ſhoot up with great celerity to the ſame point. This darting and flaſhing, together with an undulating motion from all ſides of the horizon he obſerv'd till 12 o'clock; and he was inform'd by others, that it continued till 4 in the morning. The moſt conſiderable rays proceeded from the eaſt and weſt: Next morning there was a fog.

As to the cauſe of this phenomenon, the Dr. imagines it is owing to a thin cloud, compos'd of a ſulphureous exhalation hanging over us in the air, at a conſiderable height, parallel to the horizon; the length of it being very great from eaſt to weſt nearly; the breadth of it (at firſt) not ſo great, but what we might ſee the ſtars from under it to the north and ſouth: The north ſide of it, he ſuppoſes, firſt took fire, and ſhot its ſtreams or flames perpendicularly upwards, which being undiſturb'd by winds, muſt appear ſtreight and pointed at the top; the baſes muſt form an arch, according to the rules of perſpective: For, he thinks, an horizontal right line of a vaſt length, and at a great diſtance from us (ſuch as he takes the northern edge of this luminous cloud to have been) ſeen at a conſiderable height in the air, muſt appear bent down into an arch. On a ſudden the fire propagated itſelf to all parts of this vapour: The whole heavens muſt then appear covered with the ſame ſtreams, which tho' really parallel to one another, muſt appear bent into a *cupola*. The ſhooting and darting of theſe flames, and their concourſe, together with a ſmoke proceeding from them, muſt form that confuſed cloud, which was obſerved in the center of this canopy. As to the regular diſpoſition of the colours in every ſtream he leaves others to determine. The red, he thinks, appeared at the right hand of each of them. Somewhere in the *Phil. Tranſ.* the Dr. met with an obſervation of an *aurora*, in which the ſtreams were only coloured where they met, or croſſed each other: Whether the light of one ſtream, paſſing thro' another, may not be ſeparated into colours by refraction,

he will not determine. If the altitude of the top of the arch in the north had been taken where the Dr. was, and at the ſame time at another place upon the ſame meridian, whoſe diſtance is known, from thence he imagines, the height of the cloud (as he calls it) might have been calculated.

An Account of the ſame Phenomenon; *by Mr.* John Hadley. Phil. Tranſ. N° 395. p. 146.

MR. *Hadley* only takes notice of a few particulars in the remarkable northern light of *October* 8. that have either been omitted by others, or that, by ſome remarkable circumſtances attending them, ſeem moſt likely to be of uſe to ſuch as employ their thoughts in attempting to diſcover the nature and cauſes of theſe phenomena.

The firſt ſight he had of this appearance was about half an hour after 7 o'clock in the evening; at which time it had nothing remarkable to diſtinguiſh it from thoſe others, which had been obſerved almoſt every evening for ſome time, only a duſky redneſs, ariſing from the weſtern extremity of the luminous arch; and that at the ſame time there was obſerved another ſimilar hazy arch, low to the ſouthward, fainter, but more ſteady than that to the north. He judged the higheſt point of it to be ſomething more elevated than the ſun at noon about the winter ſolſtice

In a ſhort time after, the northern arch was riſen conſiderably higher from the horizon, and continued to advance towards the zenith, till 8 o'clock; when in one part it paſſed among the uppermoſt ſtars of *Caſſiopeia*, and in another cloſe below the bright ſtar in the *harp*. The heavens underneath it looked clear, and of a dark blue colour, having no reſemblance either of dawn or duſky cloud; and the pyramids of light ſeemed to ſpring immediately out of the pure ſky. The arch itſelf was very irregular, being full of notches, ſome larger, ſome ſmaller. The duſky red on the weſt was changed to a light crimſon, and was anſwered by the like colour on the eaſt: The rays, iſſuing from both extremities were thick and bright, appearing as if there were ſeveral, one behind another. They were alſo generally longer than the reſt, and pointed conſiderably to the ſouth of the zenith. After 8 o'clock the northern arch retired again downwards, till it came among the ſtars of the *great bear*; when the whole ſcene was changed on a ſudden, and rays were darted up from all ſides, and formed that crown, or ſtar-like figure, which has been ſufficiently deſcribed.

The

The intermediate area (left between the innermoſt extremities of the rays, coming from different quarters, which, very rarely, if ever joined) was of an irregular figure, commonly inclining to an oval, whoſe longeſt diameter lay eaſt and weſt. Sometimes it appeared as clear ſky, at other times filled with a thin white cloud, and that cloud was often divided into two parts, by an uneven crooked line, running likewiſe eaſt and weſt.

The rays, which immediately ſurrounded this void ſpace, were of no great length, and very unſtable: Yet two or three times, when they continued ſteady enough to afford an opportunity of conſidering them attentively, their outermoſt extremities were ſenſibly carried ſouthwards, the center itſelf remaining, as to appearance, fixed.

The ſouthern quarter was filled with continual flaſhings of light. Theſe followed one another very quick, and were propagated upwards from the abovementioned arch with great ſwiftneſs, each of them leaving in ſome parts of the ſpace it paſſed thro', a faint and very tranſient whiteneſs, which immediately vaniſhed, and was quickly renewed, uſually in the very ſame track, by the next ſucceeding flaſh. Yet none of thoſe tracks were in any degree direct and uniform; but all very irregular and broken.

The central figure ſometimes diſappeared for a while, and then returned again. Whether it always retained the ſame ſituation with reſpect to the horizon, he could not depend on the exactneſs of his obſervations ſo as to determine. Theſe were as follows.

About ½ an hour paſt 8, the center, as well as he could judge by his eye, was very near a ſtar of the fifth magnitude, placed by *Hevelius* at the end of the *Lizard*'s tail, whoſe preſent *R. Aſcenſ.* is about 331° and Declin. 36° and ½ N. At 9 o'clock it was at the northern point of an *Iſoſceles* triangle, whoſe baſe was a line joining the ſtar in *Pegaſus*'s *ſhoulder*, called *Scheat*, and the brighteſt of thoſe in his *knee*; the perpendicular from the center being in proportion to the baſe, about 3 to 2. At $9^h\ 15'$ the triangle formed between that and the two abovementioned ſtars was become right-angled at *Scheat*, the diſtance not being much altered. At 10 o'clock it was directly between the zenith and *Andromeda*'s head at a diſtance from this ſtar, not ſenſibly different from what it had kept from the northermoſt of the two abovementioned.

According to the firſt of theſe obſervations, the central point muſt have been very near the meridian, and about 15° ſouth of the

the zenith of the place where Mr. *Hadley* was, which is a few minutes directly north from *London*. The three last agree pretty well with one another, to carry it between two and three degrees farther south, and to give it a perpendicular distance of three or four degrees of a great circle from the meridian eastwards.

In the remarkable appearance of this kind, which happened *March* 6. 1715-6. Mr. *Hadley* observed the like center at near ½ an hour after 7 o'clock in the evening, to be something nearer the zenith, than the bright star in the northern head of the *Twins*, and to be more easterly by about ½ the distance between that and the star in *Pollux*'s head. By comparing this observation with the situation of the star at that time, the center appears to have been about 16 or 17 degrees from the zenith, and about two or three degrees distant from the meridian circle towards the east.

The same Phenomenon *observed at* Geneva; *by M.* Calandrini. Phil. Transf. N° 395. p. 150. *Translated from the* Latin.

ON *Saturday* the 19. of *October* N. S. at ½ an hour after 6 o'clock in the evening the *aurora borealis* began to appear; a phenomenon pretty common in *England*, but according to the old people at *Geneva*, a thing entirely new; but this was so remarkable and so different from the common circumstances, frequently described in the *Phil. Transf.* that M. *Calandrini* gives the following account of it.

First, at the abovementioned time, a bright arch appeared, whose center was in the north of the horizon, its extremities extending towards *Arcturus* in the west, and the *Pleiades* in the east, and its altitude 40°; it was as bright, as if the sun were just rising in the north; but this is no new thing.

After 7 o'clock, flames of a violent burning, as it were, or continual flashes of lightning entirely possessed the place of the whitish arch: From the horizon they shot up towards the heavens like the real flames of a furnace; the extremities of the arch frightened the common people with their ruddy or dusky flashings; and the flames themselves seemed to terminate in a black smoke: So that the old women believed the day of judgment was come. This burning in the heavens lasted upwards of an hour; and the scene changing, columns were observed to shoot from the horizon up to the zenith: From the abovementioned basis there darted (the *crown* setting at that time in the extremity

extremity of the phenomenon, and the inferior horn of the *bull* rising in the other) columns, more or less broad, and 60 or 70° high, for three or four minutes; which either gradually evanished, or first changed into a fiery colour, and after some time quite ceased. At that time the *aurora borealis* shone as bright, as at first, to which, before 10 o'clock, their succeeded much stronger flames than before; these ceasing, more lasting columns were observed about 11 o'clock, and generally 120° high: Again the *aurora borealis* appeared, then the flames, and afterwards the columns; and in this manner, continued till three o'clock in the morning, when something of the phenomenon was still observed.

The stars of the *great bear* had a brightness but little superior to that of the faint light, which, as has been said, illuminated the northern part of the heavens. M. *Calandrini* was informed, that the phenomenon appeared in the same manner at *Vesontium*, 90 miles from *Geneva*.

In the mean time one of those meteors, called *falling stars*, was observed to have a greater brightness than the phenomenon itself.

It is affirmed, that these *northern lights* are formed by the reflection of the light of the sun from the northern frozen parts of the atmosphere: But how such remarkable flames can be explained M. *Calandrini* does not see: If this phenomenon be supposed to arise from the accension of exhalations, the *aurora borealis*, that accompanies the phenomenon, the columns, the duration of the phenomenon, and its continuing in the same place, will be the grand difficulty.

A preternatural bony Substance found in the Cavity of the Thorax; *by Mr.* William Giffard; *and a farther Account thereof; by Dr.* Rutty. Phil. Trans. N° 395. p. 152.

MAY 10. 1726. Mr. *Giffard* opened the body of a person who died of a *peripneumonia*; in the right side of the *thorax* he found an osseous substance, about a quarter of an inch thick, six inches long, and three inches broad, extending itself under the third, fourth, fifth and sixth ribs, closely and strongly adhering to the *periosteum* of the ribs and proper tunicle of the inner intercostal muscles, by fibres, which shot from a strong and thick intermediate membrane, which closely adher'd to its outer surface or back, and was likewise continued over its inner surface, thereby forming a *capsula* for this preternatural substance:

ſtance: Its upper edge lay immediately below, and was contiguous to that part of the ribs, where they become cartilaginous: The membrane, that adhered to, and covered it, continued thick for ſome diſtance from it, and gradually became thinner, and was at length loſt in the *pleura*; whence he judged this extraneous body to have been formed between the *lamellæ* of that membrane. The lungs adhered ſo ſtrongly to its inſide, that upon ſeparating them, part remained thereto: Upon cutting into it, he obſerved all the cells filled with a thin, but ſomewhat digeſted *pus*.

Upon farther enquiry, he found on the ſame ſide, towards the back, another ſubſtance, but perfectly bony, envelop'd as the former, in a ſtrong and thick membrane, and tied by the fibres, which ſhot from it, to the ſubſtance of the *vertebræ*, and to the fourth, fifth, ſixth and ſeventh ribs and intercoſtal muſcles.

Its back, or outſide, was convex, correſponding to the concave ſurface of the ribs, which had made indentures on that part of it, where they preſſed; its inſide was concave as the ribs are; the right lobe of the lungs ſtrongly adhered; part of which for that reaſon remained to it after ſeparation; its length was about ſeven inches, and its breadth from edge to edge about three inches; its thickneſs in ſome parts about $\frac{1}{3}$, in others $\frac{1}{4}$ of an inch; it had, as the former, a thick membrane running from it, which, becoming gradually thinner, was at length loſt in the *pleura*; whence he judges this extraneous ſubſtance was formed as the abovementioned.

This patient was troubled for ſome years with a ſhort cough, which latterly was accompanied with a difficulty of breathing, and great labour in inſpiration; and ſome time after, with a weight and pain on the right ſide; which ſymptoms, increaſing more and more, brought on the *peripneumonia*, of which he died in a few days. The riſe and ſucceſſion of thoſe different ſymptoms proceed naturally from the formation and growth of this preternatural body, as well as the particular part of the *thorax*, where it was lodged.

The Leaf of a Plant *lodged in a Piece of* Amber; *by Dr.* Breynius. Phil. Tranſ. N° 395. p. 154. *Tranſlated from the* Latin.

WHoever ſhall have carefully examined the *Muſæum*'s of the curious, and the amber-rarities contained therein, and diligently peruſed the authors, that have treated on this

 ſubject,

subject, he will, Dr. *Breynius* thinks, acknowledge with him, that amongst natural bodies included in amber, those from the vegetable kingdom are the most uncommon, and of these, the parts of the more perfect plants, as the leaves, pods, flowers, &c. if any such be found, are the most so.

The reason of this, doubtless, is, that, according to the opinion of the moderns, amber is naturally prepared in subterraneous places, to which the parts of vegetables (as growing on the surface of the earth) do with difficulty, and but accidentally, or very rarely reach; while insects, tho' inhabitants of the air, in order to defend themselves from the cold, and other inclemencies of the weather, or for some other reason, frequently and spontaneously retire into chinks, *hiatus*'s, and subterraneous cavities, and there hasten to their intombing; and where they are intangled, involved and suffocated, in the amber which is still in a liquid state, and with which they harden, and so are embalmed for eternity.

One *Philip Benlows* shewed the Dr. a glebe of this kind, which included a leaf, and which he valued at upwards of 30 florins.

It was almost of an oval, but compressed figure, and of the bigness represented Fig. 2. Plate IV. it was $\frac{1}{4}$ of an inch thick, and of that kind, which, from its resemblance to the colour of the wine of that name, is called *Falernian* amber; it was pretty clear and pure, and without affording the least suspicion of a cheat; quite thro' the middle lengthwise, it included an expanded leaf of the *pennate* kind, or according to others of the *alate*, tho' not so properly; obscure, it is true, but of a shining gold-colour, it yielded a very agreeable sight by the reflection and refraction of the rays of light: This leaf was not entire, but broken off at both extremities, as represented in the Fig. and it consisted of five oblong small leaves, somewhat pointed on both sides, in conjugations, almost equally distant from each other on the common rib; some of these small leaves were worn off and broken: The leaf itself had an horizontal position, which is common in that kind of leaves, only that the small leaves by their obliquity deflected a little therefrom: But the conjugations of the small leaves did not at all seem posited across; tho' that always obtains in such as botanists call the *conjugate*: So that the Dr. no longer doubted, but that this leaf was of the compound *pennate* kind: But of what particular species of plants was hard to determiue; because several species of this family have leaves so much resembling one another, that

tho'

tho' fresh, it is a hard matter to distinguish them; add to this, that even the little veins of the small leaves did not appear to the armed eye, as having been obliterated by the amber in its liquid state, and incrustated therewith, as it were. It very nearly resembled *securidaca secunda Clusii*, or *coronilla herbacea*, &c. *Tournefortii*, which is pretty common among the thickets of *Prussia*. On one side a spider was pretty distinctly seen between two of the small leaves; and on the other a small fly, but these could not be seen without a glass.

The Dr. does not remember to find in any author mention made of a leaf of the *pennate* kind included in amber, excepting in *Michael Mercatus*'s curious work, entituled *Metallotheca Vaticana*, published by *Joh. Maria Lancisi*; where among other figures of elegant glebes, containing a frog, a smaller fish, lizard and other insects, there is one, that includes such a *pennate* leaf somewhat less, but more curious and elegant, because entire, and of eight conjugations, the last small odd leaf terminating the extremity of the rib; and this greatly resembles the small and tender leaf of *coronilla herbacea, flore vario Tournefortii*; tho' it may very well be referred to *Onobrychis secunda Clusii*, which is likewise a native of *Prussia*.

The same *Mercatus* in the same place delineates another glebe, which includes a small jagged leaf of some plant, probably of the umbelliferous kind.

However, since the Dr. had not seen the said glebes, tho' very accurately delineated in the abovementioned treatise of *Mercatus*; and besides, since the figures of a large frog, a small lizard and fish gave him no slight grounds of suspecting some cheat; he would not take upon him to answer for their being genuine, because it is very well known, that such bodies, may be so artfully included in amber, as to impose upon the less knowing, and the cheat not be discovered but by the most skillfull and accurate observers.

An Eclipse *of the* Sun *at* Padua Sept. 25. 1726, N. S. *by M.* Polenus. Phil. Transf. N° 395. p. 157. *Translated from the* Latin.

app. time. h.	′	″	dig. eclipsed.	Phases
5	25	25		No beginning of the eclipse appeared at this time, and afterwards thick clouds came on.
	29	5	1	
	31	48		The greatest solar *macula*, that was to be seen, and situated between two smaller ones, was covered.
	34	27	2	
	40	43	3	
	44	20	3 ½	
	47	15	4	
	48	12	4 ¼	The eclipse still increased on the sun's disk, as it appeared thro' the clouds; but as the clouds became thicker, and the sun hastened to the horizon, M. *Polenus* could observe nothing farther.
	49	00		

An Eclipse *of the* Moon *at* Padua October 10. 1726; *by the same.* Phil. Transf. N° 395. p. 158. *Translated from the* Latin.

app. time h.	′	″	Phases
16	16	44	The *penumbra* dilute.
	18	44	The *penumbra* thicker.
	21	19	The shadow at the limb of the moon.
	31	35	It touches *Mare humorum*.
	35	47	It touches *Grimaldus*.
	38	34	It is distant from *Tycho* a diameter thereof, and covers a third part of *Grimaldus*.
	50	40	It almost touches *Pitatus*.
17	3	41	It covers *Lansbergius*.
	7	45	It touches *Reinholdus*.
	15	56	It touches *Fracastorius* and *Galilæus*.
	25	53	It touches *Mare fœcunditatis*.
	39	6	The shadow is near *Reinholdus*, and covers the third part of *Mare fœcunditatis*.

app.

app. time.			
h.	′	″	
17	46	0	*Grimaldus* emerges.
	54	53	*Grimaldus* was diſtant from the ſhadow its entire greater diameter.
18	5	44	*Gaſſendus* was obſerved entirely emerged, and *Mare fœcunditatis* about half, between the opening clouds.
			Theſe clouds, afterwards collected at the horizon, entirely overcaſt the weſtern part of the heavens; and the moon appeared no longer.
			This obſervation was made with a very good teleſcope ſeven *Paris* feet in length.

Two newly diſcovered Arteries, *going to the* Ovaria *in* Women; *by Mr.* Ranby. Phil. Tranſ. N° 395. p. 159.

IN *Phil. Tranſ.* N° 387. Mr. *Ranby* communicated to the *Royal Society* a deſcription of two arteries, which ariſing from the *aorta*, and ſending branches to the *glandula renalis*; then deſcend to the *teſtes*; and he has ſince diſcovered the ſame arteries in women, deſcending in the ſame common *capſula* with the ſpermatic artery and vein, to the *ovaria*. Theſe arteries very probably are what the late excellent *Valſalva* took for excretory ducts of the *glandulæ renales*; the diſpoſition and progreſs of theſe very much reſembling what has been aſcribed to thoſe ſuppoſed ducts.

An Eclipſe *of the* Moon *at* Rome October 31. 1724, N. S. *by S.* Bianchini. Phil. Tranſ. N° 396. p. 174. *Tranſlated from the* Latin.

True time after midnight.			Phaſes of the eclipſe
h.	′	″	
3	15	40	The beginning of the true ſhadow (as far as it could be diſtinguiſhed from the *penumbra*) now begins to appear on the moon's limb, in that part of her diſk, cut by a radius drawn from her center thro' *Ariſtarchus*.
3	32	40	Since the *maculæ* could not be diſcerned but with ſome difficulty by reaſon of clouds, he by means of a micrometer took care to define the

True

True time after midnight. h.	′	″	Phases of the eclipse
			quantity of the diameter eclipsed: Now there were about three dig. eclipsed; for, the portion of the diameter A B (Fig. 4. Plate IV.) not eclipsed, is 14 such parts, whereof the whole diameter of the moon is 18 and ½.
3	39	40	The sky having cleared up a little, the limits of the shadow seem to pass thro' *Reinholdus*; the portion of the diameter A B, not eclips'd, is 13 parts in the micrometer.
3	50	40	The limits of the shadow are (in an inverting telescope) a diameter of *Grimaldus* below the said *macula*; the limits of the shadow likewise pass thro' *Eudoxus* and *Aristoteles*.
3	54	40	The portion of the diameter A B, not eclips'd, is 9 parts of the micrometer.
4	6	10	The limits of the shadow pass thro' the centre of *Mare Crisium*, and touch the limb of *Grimaldus*. The portion of the diameter A B not eclipsed is equal to 8 parts and ½ in the micrometer.
4	7	53	*Sirius* comes very clearly to the meridian.
4	12	40	Now *Mare Crisium* is entirely covered, and the centre of *Grimaldus* is in the limits of the shadow: The portion of the diameter A B, not eclipsed, is 8 parts of the micrometer.
4	20	0	The portion of the diameter A B, not eclipsed, is 7 and ⅔ parts of the micrometer.
4	25	0	The portion of the diameter A B, not eclipsed, is about 7 parts and ¼ of the micrometer: *Grimaldus* entirely emerged, as also *Ricciolus*; the limits of the shadow pass over the edge of *Mare Nectaris*.
4	41	0	*Galilæus* emerged.
4	52	0	The portion of the diameter A B is 8 parts of the micrometer.
4	55	30	*Aristarchus* begins to emerge.
4	56	30	The centre of *Aristarchus* emerges.
4	57	30	*Aristarchus* entirely emerged.
4	59	20	The centre of *Copernicus* emerges.
5	0	10	*Copernicus* entirely emerged.

True

True time after midnight.			Phases of the eclipse
h.	′	″	
5	8	30	*Eratosthenes* emerges.
5	18	50	*Helicon* begins to emerge.
5	24	50	The portion of the diameter A B, not eclipsed, is 12 and ⅓ parts of the micrometer.
5	25	50	*Plato* begins to emerge.
5	26	55	*Plato* entirely emerged.
5	28	20	*Plinius* emerged.
5	33	50	The portion of the diameter A B not eclipsed is 14 or 14 and ½ parts of the micrometer: Wherefore 3 dig. are still eclipsed.
5	38	50	The former limb of *Mare Crisium* begins to emerge.
5	40	50	The centre of *Mare Crisium* emerged.
5	44	50	*Mare Crisium* entirely emerged.
5	48	50	The end of the true shadow, which goes off the moon's limb about the point defined by the radius, drawn from the center thro' *Cleomedes*.

Eclipses *of* Jupiter's Satellites, *at* Rome, Albano, *&c. together with the Longitude of* Lisbon; *by the same.* Phil. Trans. N° 396. p. 176. *Translated from the* Latin.

True time after midnight.			
h.	′	″	1724
2	48	30	*June* 8. At the castle of *Viscardi* above *Bolsena*, almost in the same meridian with the latter, *Jupiter*'s second satellite B (Fig. 5. Plate IV.) emerged out of his shadow, and appeared as represented in the Fig. with the other satellites.
3	0	30	The third satellite C enters *Jupiter*'s limb over against his middle *belt*.
3	26	20	The light of the innermost satellite A begins to diminish.
3	27	10	It entirely immerged; the other satellites B D, and their *belts* appearing plainly.

True

True time after midnight.			
			June 24. At *Rome*, both the immerſion of the firſt and ſecond ſatellites of *Jupiter* was obſerv'd, as repreſented Fig. 6.
1	39	0	The light of the ſecond ſatellite B begins to weaken.
1	40	20	Entirely immerged.
1	41	50	Now the light of the firſt ſatellite A begins to diminiſh.
1	42	50	Its light entirely diſappears, while the light of the other ſatellites and their *belts* appeared very diſtinctly.
3	24	29	*July* 1. at *Rome* in the morning, the entire immerſion of *Jupiter*'s innermoſt ſatellite was very diſtinctly obſerv'd; about 55′ before, its light began to grow faint, while the other ſatellites and their *belts* appeared diſtinctly, the ſky being very clear.
	40	45	*Aug.* 18. At *Rome Jupiter*'s innermoſt ſatellite began to emerge out of his ſhadow, and it ſhone with its full luſtre, as repreſented at A (Fig. 7.) between the planets limb and the ſecond ſatellite B, which was diſtant from *Jupiter*'s limb about a ſemi-diameter of his diſk. The ſatellite A emerged between the two inferior belts of *Jupiter*, in an inverted poſition in the teleſcope.
			The obſervation was very diſtinct and accurate, the ſky being very clear.
11	25	55	*Sep.* 23. At *Rome Jupiter*'s innermoſt ſatellite begins to emerge.
11	27	5	It entirely emerged.
9	53	8	*Oct.* 11. At *Albano* the innermoſt ſatellite begins to emerge.

From ſeveral obſervations made at *Rome* and *Lisbon*, the difference of the meridians is found to be 1h. 28′ of time or 22°, which difference ſeems to be either entirely true, or very nearly ſo.

An Eclipſe *of the* Moon *at* Albano Oct. 21. 1725, *N. S. by the Same.* Phil. Tranſ. N° 396. p. 179. *Tranſlated from the* Latin.

True time. h. ′ ″	Phaſes.
	S. *Bianchini* made uſe of pendulum-cocks, which, by repeated trials for ſeveraldays, were adjuſted to the true meridian.
	At the moon's ingreſs into the ſhadow, the eaſtern parts of the heavens were ſo overcaſt with clouds, and a ſoutherly wind did ſo conſtantly gather new ones, that during the whole time of the immerſion of the moon's body into the ſhadow, he could ſcarce direct his teleſcope towards it three or four times, and that by ſnatches only.
6 15 0	About this time the ſhadow was obſerv'd to touch the moon's center: But ſince the *maculæ* could not be diſtinctly defin'd in that ſhort time, in which the moon's diſk was ſeen thro' the interſtices of the clouds, he could not preciſely determine this phaſis of ſix digits of her diameter eclipſed; tho' it differ a little from 15 minutes after 6 o'clock in the evening.
6 45 0	The total immerſion happened about this time, in ſo far as he could judge during the two or three minutes time, in which he ſaw the moon pretty diſtinctly.
	After the total immerſion the moon's diſk appeared reddiſh from the refraction of the rays of light in the earth's atmoſphere; but of a more diluted colour in that part of the limb the ſun went off laſt.
	It afterwards growing ſerene, he had an opportunity of perfecting the obſervations of the emerſion.
8 20 0	The moon's diſk is of a whitiſh colour on that limb which is next to be illuminated: Yet the direct light of the ſun did not touch her diſk.

True time.			Phases
h.	′	″	
8	25	0	The moon's limb becomes ſtill clearer; but ſtill it does not emerge out of the true ſhadow.
8	27	0	Now the moon's limb firſt begins to recover its light in that part of the circumference ſituated between *Grimaldus* and *Galilæus*; which are ſtill covered.
8	29	40	The extremity of the light touches the firſt limb of *Grimaldus*.
8	30	40	*Grimaldus* entirely emerged.
8	31	36	*Galilæus* emerges.
8	35	40	*Ariſtarchus* begins to emerge.
8	36	6	*Ariſtarchus* entirely emerged.
8	48	20	The former limb of *Copernicus* begins to emerge.
8	49	50	*Copernicus* entirely emerged.
8	51	20	*Plato* entirely emerged.
8	54	0	The former limb of *Tycho* begins to emerge.
8	56	0	*Tycho* entirely emerged.
8	59	0	The ſubtenſe of the arches CAD and CFD (Fig. 1. Plate V.) was found to be 22 ſuch parts of the micrometer, whereof the moon's diameter is 24 in a teleſcope of 11 *Roman* palms in length; but AB is 12 ſuch parts.
9	2	0	*Menelaus* 25 (with which number the *macula* is mark'd in the figure of the moon, publiſh'd by the *Royal Academy* at *Paris*) emerges.
9	5	0	The bright *macula* ſituated before *Plinius* emerges.
9	6	0	*Hermes* emerges.
9	50	0	*Plinius* emerges.
9	16	0	*Poſſidonius* (27) begins to emerge.
9	18	0	The former limb of *Mare Criſium* emerges.
9	25	0	*Mare Criſium* entirely emerged.
6	24	0	*Langrenus* (39) emerges.
9	24	30	The extremity of the ſhadow is ſtill ſeen on the moon's limb.
9	25	0	The end of the true ſhadow.

Obſer-

Observations on the Spot Plato, *in the* Moon, Aug. 17. 1715. *N. S. by the Same.* Phil. Transſ. N° 396. p. 181. *Tranſlated from the* Latin.

THE raiſed edges in the circumference of the ſpot were enlightened by the ſun, and they ſhone with their uſual brightneſs: The bottom of the ſpot appeared dark, the rays of the ſun not having hitherto reach'd it; but a light not ſo white, nay ſomewhat reddiſh, was projected thro' the middle area of the ſpot, as in Fig. 8. Plate IV. juſt as if there were ſome hole in that part of the edge A oppoſite to the ſun, thro' which a ray of the ſun enter'd.

There are two cauſes aſſign'd of the above-mentioned effect; either a hole in that part of the edge oppoſite to the ſun; or the refraction of ſome ſolar ray in the ſummit of that edge, whence the ray might pervade the internal parts of the ſpot itſelf. Both, it is true, are probable, and both equally confirm, that there is an atmoſphere round the moon, not unlike ours; whether we admit of a hole, thro' which a ſolar ray may enter, and here, not be ſeen in the obſcure cavity of the *macula*, unleſs it become manifeſt by exhalations reflecting the light, or whether we admit of refraction; which cannot poſſibly happen without a groſſer medium.

S. *Bianchini* made theſe obſervations an hour after ſun-ſet, on the *Palatine* hill, with a very good teleſcope of S. *Campani*, 150 *Roman* palms in length.

A Remark on the new Opinion relating to the Forces *of moving* Bodies, *in the Caſe of the Colliſion of* Non-elaſtic Bodies; *by Mr.* John Eames. Phil. Tranſ. N° 396. p. 184.

A Variety of experiments have been made, and reaſoning us'd both in *England* and *France*, to prove the truth of the common opinion; but they do not entirely ſatisfy all the gentlemen on the other ſide of the queſtion. The ingenious profeſſor of Mathematics and Philoſophy at *Utrecht*, in the preface to his *Epitome elementorum Phyſico-mathematicorum*, publiſh'd in 1726, tells us ' in computing the forces of moving bodies I ' have embraced the opinion of the celebrated M. *Leibnitz*, ' *Huygens*, *Polenus* and '*SGraveſande*, and bid adieu to the ' old opinion, which hitherto I maintain'd and taught; and ' that, notwithſtanding the arguments of very learned men, ' both in *France* and *Britain*, who maintain it: And when

'the experiments, describ'd by *Polenus* and *'SGravesande* are
'examined, they so evidently evince the forces of striking bodies
'to be in a ratio compounded of the square of the velocities,
'and of the simple ratio of the masses, that we must submit to
'them, unless we would contradict the plainest evidence.'

Mr. *Eames* examines the truth of the new opinion in the case here propos'd, *viz.* as to *the forces of striking bodies*; and endeavours to prove from their own principles, that it cannot be true in all the cases of non-elastic bodies.

It is allow'd, that the common rules of finding the velocities of non-elastic bodies, after the stroke, are true: For, thus the ingenious M. *'SGravesande* in paragraph 251 of his *Supplementum Physicum*, tells us: From this principle (*i. e.* by mul-
'tiplying the mass into the velocity) philosophers have deduced
'these very rules N° 234, *seq.* 237, *seq.* which I have several
'ways deduced from my principles: But what happened in this
'case is surprising, one error destroy'd another, and a twofold
'error led me to find the truth; they followed a false princi-
'ple in computing the forces, and suppos'd what is not at all
'consonant with truth, *viz.* that bodies lose nothing of their
'force by the intropression upon the parts, and overcoming
'their cohesion.' Now the rule, for finding the common velocity of non-elastic bodies, moving the same way after the collision, is, to divide the sum of the quantities of motion in the two bodies, by the sum of the quantity of matter.

It is also granted, 'that these bodies cannot mutually act
'upon each other with the quantity of motion common to both,
'*sect.* 215. the stroke, therefore, depends on the relative velo-
'city, which remaining, the intenseness of the impulse will be
'the same, in whatever manner the absolute velocities may vary,
'On this intenseness depends the intropression of the parts,
'which, therefore, will always be the same, if two bodies im-
'pinge with the same relative velocity, whatever be the velo-
'cities they move with.'

These principles furnish us with an argument against the new opinion: For, if it be true, then equal causes may have unequal effects, and that in their own sense of an effect: The proof shall be taken from instances of the effects of the collision of non-elastic bodies, whose relative velocities shall be always equal.

Let A and B represent two non-elastic bodies of equal quantities of matter; and let B be at rest, while A moves towards it with eight degrees of velocity.

Here

Here the common velocity after the ſtroke will be half the velocity of A before the ſtroke, *i. e.* four degrees; conſequently, the force in B, thus communicated by the ſtroke, will be as its ſquare, or 16.

Let B move forwards with two degrees of velocity, and A follow it with 10 degrees; the relative velocity will be 8, as before; conſequently, by their own principles quoted above, the ſtrokes in both caſes are equal: The velocity in B, after the ſtroke will be half the ſum of the velocities before the ſtroke, or ſix degrees by the common rule.

According to the new opinion, the forces being as the ſquares of the velocities, the force of B before the ſtroke will be to its force after the ſtroke, as the ſquare of 2 to the ſquare of 6, that is, as 4 to 36; ſubduct the force in B before the ſtroke, from the force it has after the ſtroke, and you have the degrees of force communicated by the ſtroke; which, if this opinion were true, would be 32, juſt double the number of degrees communicated by the ſame force in the former inſtance, which was but 16. Thus equal ſtrokes produce unequal effects in our ſenſe of effects.

The following table exhibits ſeveral other inſtances: In the three firſt columns you have the velocities of the two bodies both before and after the ſtroke; in the two next you have the force in B both before and after the ſtroke; and in the ſixth the difference of thoſe forces, or the different degrees of force effected by the ſame ſtroke; and in the laſt column, the proportion of thoſe forces, or effects of the cauſe or ſtroke.

The velocity in A	B	B	The forces in B		Force communicated by the ſtroke.	Proportion.
8	0	4	0	16	16	1
10	2	6	4	36	32	2
14	6	10	36	100	64	4
18	10	14	100	196	96	6
22	14	18	196	324	128	8
26	18	22	324	484	160	10
before		after the ſtroke.	before	after the ſtroke		

If it be ſaid that Mr. *Eames* has not conſidered the other part of the entire effect of the ſtroke, *viz.* the intropreſſion of

of the parts; he replies, this will make but a ſmall alteration in the matter; ſince the intropreſſions in all theſe caſes are equal, the relative velocities being by ſuppoſition the ſame: So that notwithſtanding, upon the whole, one and the ſame, or equal cauſes will produce unequal effects.

Remarks on a ſuppos'd Demonſtration, that the moving Forces of the ſame Body are not as the Velocities, but as the Squares of the Velocities; by the Same. Phil. Tranſ. N° 396. p. 188.

THE demonſtration runs thus; 'I conceive that the body ' C (Fig. 9. Plate IV.) impinges obliquely on the ' ſpring L with the velocity C L, as 2, the angle of inclination ' C L P being of 30 degrees, whoſe ſine C P is half the radius ' C L. I ſuppoſe the reſiſtance of the ſpring to be ſuch, that ' to bend it, there is preciſely required one degree of velo- ' city in that other body, ſhould it impinge perpendicularly: ' What then, ſhall be the conſequence after an oblique im- ' pulſe of C againſt the ſpring L? Since the motion in CL is ' compounded, as is well known, of the two collateral motions ' in C P and PL; and ſince C P, according to which the body ' directly impinges on the ſpring L, repreſents half the ' velocity of the body thro' C L, this motion thro' C P will be ' deſtroyed, when the ſpring is bent (for, it would be ' the ſame thing, as if the body C ſhould with the velocity ' C P, perpendicularly impinge on the ſpring, which by the ' hypotheſis, might deſtroy that velocity) the velocity of the ' body and the direction P L continuing the ſame: Producing ' therefore, P L to M; ſo that L M be $= PL = \surd 3$ (for, ' C L is ſuppoſ'd $= 2$) and applying in M another ſimilar ' ſpring, forming with L M the angle L M Q, whoſe ſine L Q ' $= CP = 1$; by the ſame reaſon it is manifeſt, that the body ' C, after the bending of the ſpring L, will bend the ſpring ' M, loſing the motion thro' L Q, and retaining that thro' ' Q M: Producing, therefore, Q M to N, that M N may be ' $= QM = \surd 2$; and putting there a third ſimilar ſpring, ' forming with M N half a right angle, as M N R: So that ' M R be again $= CP = 1$; in like manner it is evident, ' that the motion through M R is entirely expended on ' bending the ſpring N, the body in the mean time continu- ' ing to move with the direction and velocity $RN = 1$: In ' fine, if, with this remaining velocity, it ſhould perpendicu- ' larly impinge upon the ſpring O, it will entirely commu-

' nicate

'nicate the reſt of its force in bending it; and, therefore, 'the body itſelf will be brought to a ſtate of reſt. From 'theſe premiſes it now appears, that the force of the body 'C was ſo great, that by itſelf alone, it could preciſely bend 'four ſuch ſprings, to bend each of which apart, there is 'requir'd half the velocity of a body equal to C itſelf; conſe-'quently, ſince the effect of the former is four times greater 'than the effect of the latter, it is likewiſe evident, that the 'force of a body of two degrees of velocity, is four times as 'great as the force of the ſame, or an equal body of one 'degree.

'In much the ſame manner I might demonſtrate, that the 'body C with a velocity of three degrees may bend nine 'ſprings, to bend one of which there is one degree of velo-'city required in that body; and in general, that the number 'of bent ſprings is always the ſquare of the number of the 'degrees of velocity: Whence, therefore, it will follow, that 'the forces of equal bodies are in a duplicate ratio of their 'velocities, Q. E. D.'

1. This argument is founded entirely on the commonly receiv'd doctrine of the compoſition and reſolution of forces, and not on any deciſive experiments, that have actually been made upon this occaſion.

2. All that is prov'd from this doctrine is, that a body moving with two degrees of velocity, may be made to bend four, and with three degrees of velocity it may be made to bend nine ſimilar ſprings, each deſtroying one degree of velocity in a perpendicular direction, before its force is entirely ſpent, provided you take care to alter the directions of the motion in every ſtroke, but the laſt, after a certain manner; that had the ſame body mov'd but with one degree of velocity in one direction, and that in a perpendicular one, it would have loſt all its force at once, and bent but one of thoſe ſprings; which is far from proving the thing in queſtion.

3. To make the reaſoning on this head concluſive, the two bodies ſhould not only be equal in quantity of matter, but alike in that material circumſtance, *viz.* the direction of their motions: So that if one of the bodies move in a perpendicular direction, the other ſhould do ſo too; or if the one ſtrike in an oblique direction, the other ſhould do the ſame, and that in the ſame degree of obliquity; and laſtly, if one move in ſeveral directions, the other ſhould do the ſame. But in the caſe before us, one is ſuppos'd to move but in one direction perpen-

perpendicularly, and the other to move in three oblique directions, and but one perpendicular,

4. Let, therefore, the ſame body move always in the ſame directions, and with a ſmall alteration, the argument uſed in this demonſtration will be ſo far from proving that ſide only of the queſtion, for which it was brought, that it will equally ſerve to prove the truth of the other; namely, that the forces of the ſame body moving with different velocities are as thoſe velocities.

Let, therefore, the ſame body, inſtead of moving with two degrees of velocity, move but with one, and in the ſame directions as above; only let the ſprings be capable of deſtroying but $\frac{1}{2}$ a degree of velocity in a perpendicular direction; then by the ſame ſteps of reaſoning it will follow, that this body will now alſo bend four ſimilar ſprings before its force is ſpent: So that the ſame body moving with half the velocities, and in the ſame directions as before, bends the ſame number of ſprings; only now the ſprings make but half the reſiſtance, that thoſe in the former caſe made; therefore, the effect in this caſe, according to our way of eſtimating an effect, is but half the former effect; conſequently, the forces producing theſe effects are as two to one: But in this ratio are the velocities, with which the body moved in the two caſes; therefore, the forces are as the velocities.

Let the body move with three degrees of velocity, and it will bend nine ſimilar ſprings, each deſtroying one degree of velocity in a perpendicular direction, before the whole force is deſtroyed: So in like manner by the ſame way of reaſoning, it is as certain, that if the ſame body move with one degree of velocity, it will bend nine ſimilar ſprings, each deſtroying a third part of one degree of velocity in a perpendicular direction, before its force is deſtroyed: So that ſtill the effects, or reſiſtances overcome in the ſame directions are, according to our way of computing, as 3 to 1; and ſo in like manner their forces muſt be but in the ſame ratio of 3 to 1, as were the velocities; conſequently the forces are as the velocities.

5. Since, therefore, this proof, drawn from the doctrine of compoſition and reſolution of forces, equally proves both ſides of the queſtion, it proves too much, or really nothing at all; and is, therefore, far from deſerving the name of a demonſtration.

An

An Account of Cobalt, *from which* Smalt *is made*; *by M.* Linck. Phil. Transf. N° 396. p. 192. *Translated from the* Latin.

COBALT is a grey mineral ſubſtance, reſembling the white ore of ſilver, and white *pyrites* in colour, but ſomewhat darker, containing white arſenic and a fixed earth, and with flint and pot-aſh changing into a blue glaſs.

It is dug up in the territories of *Schneeberg* and *Anneberg*; eſpecially in the ſhafts of *Hirſch*, *Rappolt*, *Galilæa*, *Centum millia equitum*, &c. and that in peculiar veins, and ſo entirely pure, together with a very hard flint, or *quartz*, that if there happen to be found any other kinds of minerals intermixed therewith, they appear, not joined with the true ſubſtance of the cobalt, that adheres to the ſaid flint, but from other cauſes, with other veins.

The immediate and undoubted external ſign thereof is a certain mineral of a roſey colour, and ſtriated, called Cobald-blüthe, *i. e.* flowers of cobalt, which is laid up as a mineral rarity in the *Muſæum's* of the curious: There is another mineral, not of ſo roſey a colour, but paler, like the powder encompaſſing the mineral.

By an accurate compariſon of this mineral with white *pyrites*, and a ſtill more accurate one with the grey ore of copper, and in fine by a moſt accurate compariſon with the white ore of ſilver, you have a nearer ſign of it; with all which it has no ſmall reſemblance in colour, tho' there be the greateſt eſſential difference; but it cannot be either ſufficiently explained or conceived: So that one muſt frequently have ſeen it.

The immediate ſign of it, or what makes the thing itſelf evident, is the glaſs made of it; which from the *pyrites* is black, from the ſaid ore of copper, reddiſh, from the white ore of ſilver and which participates of ſome copper, is ſometimes more and ſometimes leſs blackiſh. From the true cobalt is produced *zaffera*, denoting both roaſted cobalt, and the blue glaſs pulveriſed, probably from its ſapphir colour.

1. Cobalt of itſelf when for ſome time expoſed in a heap to the open air, rain and ſun, yields a roſey efflореſcence called *flos Cadmiæ*; but not to be confounded with the flowers above deſcribed. When the former is duly extracted, it yields a very elegant red ſubſtance, and beſides, a ſmall portion of a dark green vitriol, which deſerve to be farther conſidered; but there is required a conſiderable quantity of the mineral, as alſo time,

care and patience; as to the operations, M. *Henkel* underſtood them well.

2. Spirit of nitre diſſolves it with an *impetus* and efferveſcence; for inſtance 4 or 6 parts to 1, according as the mineral is more or leſs pure: Whence there uſually reſults a ſolution of various colours; for, there is the green colour of the vitriol of copper, if it contain copper; which appears in that cobalt, called Kupfer-nikel; and whilſt crude, it has the colour of copper: Yet ſometimes there is likewiſe produced a green colour from what is entirely grey, and in no manner appears of a copper-colour, tho' approaching to that of a ſolution of pure martial vitriol; yet ſtill as to the touch it does not differ at all from copper. M. *Linck* finds, that the dark yellowiſh ſolution is beſt for making the blue glaſs; when reddiſh, it ſhews that it is impregnated with biſmuth.

3. Solutions, precipitated with an *alkali*, for inſtance, oil of tartar *per deliquium*, yield a beautiful blue powder for ſtaining porcelain; eſpecially, if the cobalt happen to be very good, and that the fire has been ſkillfully managed.

4. The acids of vitriol and common ſalt, it is true, affect the *Cadmia*; but they do not ſo much diſſolve as corrode it, eſpecially the acid of vitriol, and reduce it to a white powder: So that ſpirit of nitre is more proper to diſſolve the *Cadmia*.

5. Cobalt, when duly confined in a cloſe veſſel, a luted ſtone retort, or other ſubliming veſſel, in an open fire, ſuffers the arſenical volatile particles, to evaporate and fly off, which at firſt are ſomewhat fuliginous, nay even partake of ſandarach, and conſequently, are not entirely free from common ſulphur; but they are collected into a ſhining, reguline, foliated, ſemi-metallic form, ſuch as are produced from white *pyrites*; but when the brightneſs evaniſhes for days, nay for hours, it is an evident proof, that particles of air have an ingreſs into that ſubſtance.

6. In a roaſting furnace (Roſt-offen) where the flames touch the mineral, there ſticks to the chimney or other vents a powder (Mehl) as it were, of a whitiſh aſhy colour, which is gathered for making the white cryſtalline arſenic.

7. There remains a fixed, cineritious vitreſcible earth.

8. Biſmuth mineral (not to be omitted here, as greatly ſubſervient to make ſmalt) does in an open fire eaſily part with the ſemi-metallic ſubſtance, called biſmuth; which is ſold for other uſes; and leaves behind a greyiſh fixed ſtone, or earth, called Biſmuth-graupen.

9. The

9. The bismuth mineral, if pure, is separated as much as possible from the cobalt, strictly so called, that the bismuth may be gathered apart: But sometimes, nay frequently this last is so intermixed and entangled with it, that an entire separation is impossible: Hence in vitrification there subsides a *regulus*, called Speiffe, commonly of a white reddish colour; which yet is seldom or never such pure bismuth, as is obtained from its own proper mineral, but has among other things an adventitious fixed cobaltic earth mixed with it; and so it is mixed up again with new vitrifying compositions: Some subdivide this *regulus* into marcasite, and Glokenspeiffe or *æs caldarium*, the former standing lower and the latter higher.

10. The fixed earth, that remains, does with a great deal of difficulty or not at all change into glass; tho' M. *Linck* had kept it for 8 hours in a glass-furnace, yet it scarce impressed 2 or 3 blueish spots on a *Hessian* crucible: And since some grains of sand in the clay, of which these crucibles are made, concur, it still remains a question, whether of itself, without any additament, it may make blue glass; and this he doubts of, because the mass, where it does not touch the vessel, is greyish, and continues so.

11. That earth is not quite barren, but participates of some metal; which is easily presumed, if not from the colour of the solutions, yet from the blue glass manifestly made from it and from the cobalt, called Kupfer-nikel. But that the quantity of metal is inconsiderable, appears not only from the lightness of the earth, but likewise hence; because a metallic copper substance can by no means be obtained, either from the best native *Cadmia* or that denominated from copper: For, whatever small portion of copper it may contain; yet what it is cannot be defin'd, unless we say, that it is peculiar to *Cadmia*.

12. There is a cobalt, which even without roasting, yields blue glass, nay sometimes a more elegant sort, than when it is roasted; which is not to be learned from viewing the mineral, but by experience.

13. And in like manner is the knowledge of the degrees and continuance of the fire to be learned by experience; since one mineral requires a longer or shorter, a stronger or gentler degree of roasting than another.

14. Both the vitrification and the elegance of the colour is promoted by adding the arsenical powder, nay the crystalline arsenic itself.

15. There are not it is true, entirely particular rules as has been already hinted for treating cobalt in order to make blue glaſs: Several experiments muſt, therefore, be made not only in a private laboratory, both according to the ſeveral proportions, and to the degrees of fire and other circumſtances; but in the cobalt houſes themſelves, the aſſayer muſt by indefatigable experiments obſerve the nature of the mineral and what is join'd therewith; and ſo duly regard the compoſitions and operations, which is a very nice affair in colours; as thereby to know, either to add, take away, or otherwiſe correct them.

16. Whoever, therefore, would try cobalt for this end, muſt carefully obſerve whether it be pure, and in what degree, from ſtones and other matters; then roaſt or calcine it, till it ceaſe to ſmoke; mix with the remaining powder, 1, 2, or 3 parts of flint, or a very pure, white, flinty ſand, and one or more parts of pot-aſh, according to the degree of the fluxility of the mineral, mix them well, and put them into a crucible conical at bottom, like the **Kupfertiegel**, ſuch as copper aſſayers uſe. If it flux well, which you may try with an iron rod, and which happens ſooner or later, according to the nature of the furnace and degree of fire, take out the crucible, and ſeparate the cold glaſs from the droſs; then pulveriſe it, not in an iron veſſel, in regard it makes the colour dark, but, in a porphyry or other very hard ſtone veſſel; buddle or waſh the powder from what is of a whitiſh or greyiſh colour, and called *Eſchel* or *Schlamm*; firſt dry the one, and then the other; and afterwards paſs them thro' a very fine ſieve, again ſtamp with water the groſs parts that remain, dry them, till the whole be pulveriſed: Then compare your blue colour with thoſe ſamples of colours, whoſe degrees, orders, names, and prices, are ſettled by public authority; and then you may judge of your cobalt, and where you have made a miſtake.

The method of proceeding in the groſs, as it is called, is thus; having taken the cobalt mineral out of the ſhaft, and ſeparated it with a hammer, as much as poſſible, from all the heterogeneous matters, it is calcined in a vaulted furnace, broad and almoſt flat at bottom, which receives the flames of a fire from another adjoining wind furnace over the mineral; and one ſtirs about the cobalt earth, that the fire may act upon it every way, and cauſe it diſcharge the arſenic the better, till it fume no more: Moreover very pure flints are picked and calcined, and thrown glowing hot into cold water, that they may become the more tender; and being thus prepared they are ſtamped to a powder

a powder or ſand: Then 1 part of roaſted cobalt, with 3 parts of flint at moſt, and (that it may flux the better) with 1 part of pot-aſh, and mixed in a certain box, is fus'd in a glaſs furnace, in large pots, with an intenſe degree of fire, for 8, 10, nay 12 hours; and the ſtoaker ſtrongly ſtirs about the maſs, now fuſed, with a crooked iron rod, that the whole may flux equally: When it has attained its greateſt degree of fuſion the glaſs is laded out with a large iron ſpoon, and thrown into a trough of water: The glaſs rendered thus more friable is ſtamped with a hammer raiſed by water, and what is ſtamped is paſſed thro' a braſs-ſieve: The remaining part is farther ſtamped, and ground fine in a mill with water; this is afterwards buddled or waſhed, to ſeparate the ſmalt from the ſaline particles and other filth, and that light, whitiſh, aſh-coloured powder, called **eſchel**. At length it is put up in veſſels in centners, and ſold all the world over. As there is a very great difference in this commodity, and conſequently, in the price, ſo the veſſels, containing it, are marked with certain letters; as O. C. ſignifies **ordinair cobolt.** M. C. **Mittel cobolt.** F. C. **Fein cobolt.** F. F. C. **Fein, Fein, cobolt,** F. F. F. C. **Fein, Fein, Fein cobolt,** which laſt as it is the moſt valuable, ſo it is the moſt uncommon. The cobalt mineral thus prepared, is otherwiſe called **ſmalt,** and by the women **blaue ſtarcke,** which they commonly uſe in bleaching linen.

17. There is ſold a cobalt, that is only roaſted, and mixed with two or three parts of flint, or **quartz,** under the name of **zaffor**, or **zaffera,** for earthen ware: Where this is obſervable, that the powder becomes ſo compact in the veſſels in which it is carried, and concreted in ſuch a manner, that like a hard ſtone it muſt be broken aſunder with a hammer.

19. In fine, cobalt of itſelf does not contain ſilver: Tho' there be a vein of this kind, from which this noble metal is got; but this is plainly accidental, becauſe the ſame vein yields ſometimes more and ſometimes leſs ſilver, and for the moſt part none at all.

20. Notwithſtanding, native ſilver is frequently found over the cobalt, which argues no more than this, that that mineral is not an improper *matrix* for ſilver.

Remarks

Remarks on some **Dissertations, *published at* Paris; *by* P. Souciet *against Sir* Isaac Newtons's Chronology; *by Dr.* Halley. Phil. Transf. N° 397. p. 205.**

DR. *Halley* had put into his hands a book published at *Paris* in 1726 by *P. Souciet* against Sir *Isaac Newton's* chronology, without waiting till the book be published, and without knowing the contents thereof, otherwise than by a short extract, made at the desire of a very great person, and without intention of its being made publick. However, a copy thereof having been (as the Dr. supposes) surreptitiously obtained and carried over into *France*, the same was first translated into *French*; and then printed at *Paris* with a pretended refutation thereof by the same *P. Souciet*. Since that time, Sir *Isaac Newton* having answered, as he thought, his objections, has thereby given him a handle to publish five other dissertations against the new system of chronology, as he calls it; the first and last of which being chiefly astronomical (since the great author is no more) seem properly to fall under the Dr's. examination, as being royal astronomer.

1. The Dr. observes, that *P. Souciet* readily allows what seems to be the most exceptionable part of the whole system, *viz.* that *Chiron* the *Centaur* fixed the colures, in the ancient sphere of fixed stars, in the same places, as *Hipparchus* tells us they had been supposed by *Eudoxus* several centuries after *Chiron*. His words are those; εν δε τω ἑτερω κολουρω φησι κεισθαι του κητους την κεφαλην και του κριου τα νωτα κατα πλατος. i. e. *he says, that in the equinoctial colure the head of the whale and the back of the ram lie in Lat.* This, undoubtedly, was the position of the colure of the vernal equinox many ages before *Eudoxus*; but whether so old as *Chiron* and the *Argonautic* expedition, the Dr. does not now enquire; but only observes, that *P. Souciet* in his *Fastes du monde*, or abridgment of his chronology, prefixed to these dissertations, makes the *Argonautic* expedition 1467 years before our *æra* of the birth of *Jesus Christ*, and the taking of *Troy* 1388 years before it, which date is 120 years sooner than the *Parian* chronicle, read and published by our learned *Selden*, in his *Marmora Arundeliana*, makes it; and upwards of 500 years earlier than the times assigned by Sir *Isaac Newton*.

Now both of them making use of the same premises, it may seem strange that their conclusions should be so wide distant; and indeed, upon a prepossession, that the *Argonautic* expedi-

tion

ion, and the ſiege of *Troy* could not have been leſs than 1000 ears before *Chriſt*, the Dr. owns, he was at firſt ſomewhat pre-udiced in favour of *P. Souciet*, taking his calculations for ranted, and not having ſeen Sir *Iſaac Newton*'s work: But bſerving, that he quotes Sir *Iſaac*, as ſaying, that in conſe-uence of what *Hipparchus* has recorded from *Eudoxus*, the quinoctial colure in the old ſphere was about 7° 36′ from the irſt ſtar of *Aries*, the Dr. was reſolved to examine the matter with due attention; eſpecially ſince *P. Souciet* ſeems to triumph ver his adverſary, and to treat a man of his figure in the com-mon-wealth of learning in a very ludicrous manner; notwith-ſtanding the ſeveral fine things he ſays of him to palliate it.

The Dr. finds the diſpute to be chiefly over what part of the back of *Aries* the colure paſt: The words of *Hipparchus*, as from *Eudoxus*, are ſimply, that it paſt over the back, without ſaying over what ſtar, or over what part of the back it paſt: And the ſame *Hipparchus* ſhews, that if it paſt over the ſtar in the middle of the back, it widely differed from the ſituation thereof in his time; and conceiving thence, that the equi-noctial points might have a regreſſive motion, he was the firſt that attempted to define their motion; but having no obſerva-tions older than thoſe of *Timocharis*, made within leſs than 200 years of his own time, and very rude withal, he could not determine the quantity thereof, but conjectured it to be about a degree in 100 years; which length of time, and the more cu-rious obſervation of the moderns, have now proved to be 1° 24′; or rather 50″ *per annum*.

In a word, Sir *Iſaac Newton* takes the colure to have paſt over the middle of the conſtellation of *Aries*, and very near the ſtar in the middle of the back (*Bayer*'s ν) and *P. Souciet* will have it, that it paſt over the middle of the ſign, or *dode-catemorion* of *Aries*, reckoning the ſign to begin with the firſt ſtar of the conſtellation; and conſequently, his colure muſt paſs about midway between the rump and firſt of the tail of *Aries* (*Bayers* ε and δ) which ſituation could never be ſaid to be over the back: But while Sir *Iſaac* makes the colure but 7° 36′ from the firſt ſtar of *Aries*, which *P. Souciet* makes 15° from it, the difference 7° 24′ at 50″ *per annum*, makes 533 years difference in the reſult.

Let us now examine, when the ſtars in queſtion did actually paſs under the colure of the vernal equinox, aſſuming their places as they are in Mr. *Flamſtead*'s *Catal. Britan.* adjuſted to the beginning of the year 1690. He places the firſt ſtar of

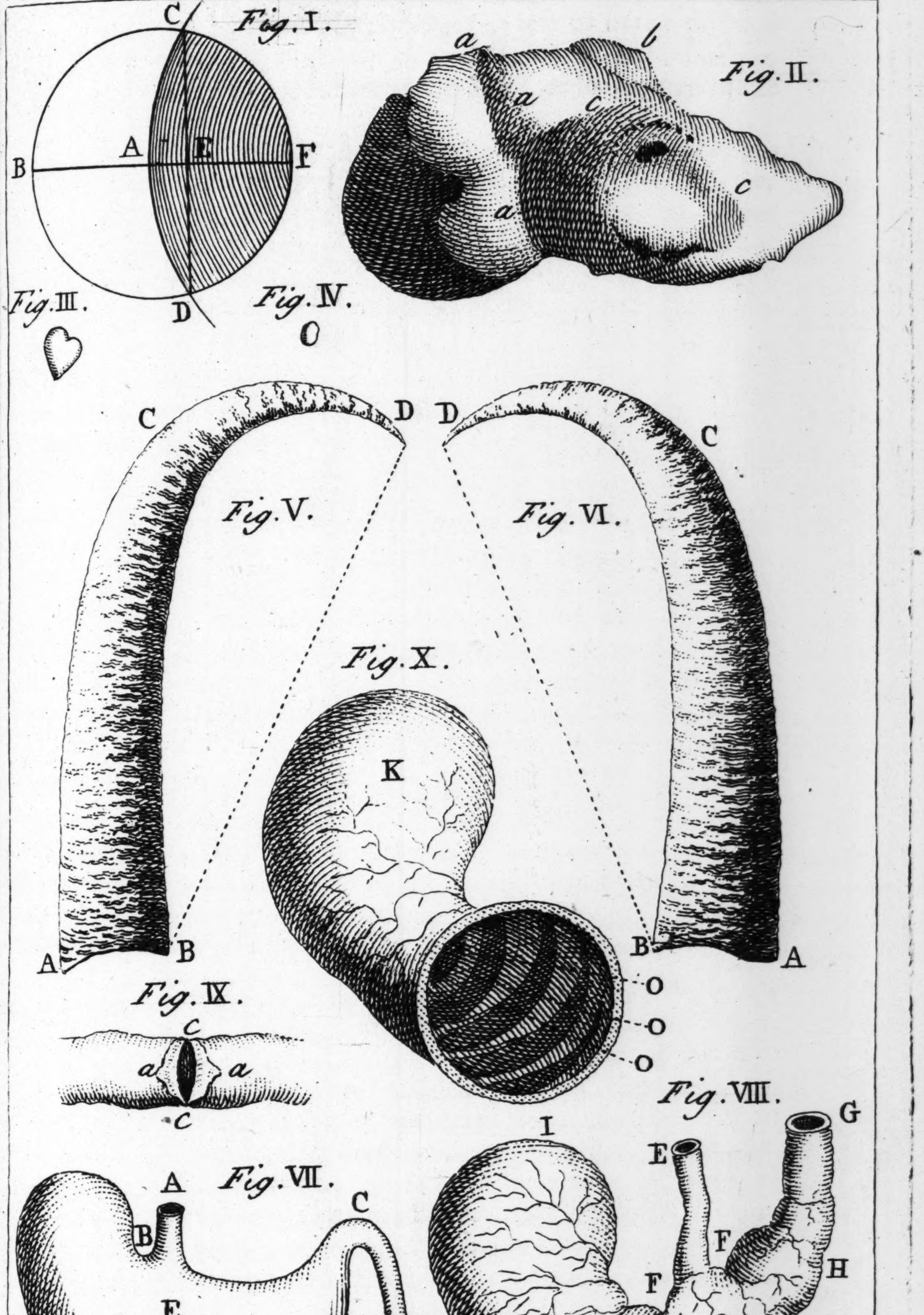
Aries

Aries in 28° 51′ of *Aries* with 7° 9′ N. Lat. And ſuppoſing the obliquity of the ecliptic 23° 29′, it will be as the radius: to the tangent of 23° 29′:: ſo the tangent of 7° 9′: to the ſine of 3° 7′ and a half, the difference of Long. between the ſtar and the point in the ecliptic, which paſt under the colure, at the ſame time with the ſtar: So that this point was in the beginning of the year 1690 in 25° 43′ 30″ of *Aries*; and therefore allowing 50″ *per annum*, the ſtar was under the colure 1852 years before the *epocha* of the *British* catalogue, that is, 162 years before our *æra* of the nativity of Jeſus Chriſt; in which very year *Hipparchus* began to obſerve the equinoxes recorded by *Ptolemy lib.* 3. *cap.* 2.

If, therefore, with Sir *Iſaac Newton* we add 7° 36′ to the Long. of the firſt ſtar of *Aries*, as it was in 1690, we ſhall have 36° 27′, which the colure moves in 2624 years; and deducting 1690 therefrom, we ſhall have 934 years before Chriſt for the *Argonautic* expedition: And if to 7° 36′ we add 3° 7′ and a half, we ſhall have 10° 43′ and a half, that is, 772 years before the firſt ſtar of *Aries* paſt the colure.

Next let us enquire, when the ſtar in the middle of the back of *Aries* (*Bayer*'s *v*) paſt the colure. Its Long. in the beginning of the year 1690 was 9° 48′ 35″ of *Taurus*, with Lat. 6° 8′: But by the foregoing analogy, the point in the ecliptic, over which the colure paſt at the ſame time with it, was 2° 40′ and a half before it, that is in 7° 8′ of *Taurus*. Now 37° 8′ give 2674 years nearly, or 984 years before Chriſt, when that ſtar was under the equinoctial colure, being but half a century earlier than Sir *Iſaac* places the *Argonautic* expedition; and ſhews, that he took the middle of *Aries*, over which the colure is ſuppos'd to have paſt, to be the middle of the conſtellation, and not of the *dodecatemorion*; and in ſo doing, no doubt, had reaſon to place this colure 7° 36′ in conſequence of the firſt ſtar of *Aries*, inſtead of 8° 17′, as it was when the ſtar in the middle of the back of *Aries* was under the colure.

But if with *P. Souciet*, you make the colure to interſect the ecliptic 15 degrees from the firſt ſtar of *Aries*, or 43° 51′ from the equinoctial point, as it was *Anno* 1690, we ſhall have the time nearly 1470 years before Chriſt; but then the colure will be very far from the middle of the back of *Aries*, and leave only his tail to the eaſtward, as it leaves the head of the *Whale* to the weſtward; ſo as by no means to agree with the deſcription we have of it from *Hipparchus*; which it



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it were to be wish'd had been more definitive, and as well circumstanciated, as what *Hipparchus* has left us of the position of the colures, in his own time, which, upon examination, the Dr. finds to be very consistent, and the observations made with sufficient care.

Thus the Dr. hopes, he has shewn P. *Souciet*, that there was no affectation of mystery, in Sir. *Isaac*'s placing the colure 7° 36′ from the first star of *Aries*, nor any occasion for his mirth, as in p. 131, 132 on that account; as also that he ought to have deducted 3° 7′ and a half out of the 15 degrees he assumes for the distance of his colure from the first star of *Aries*, which will bring him 225 years nearer to Sir *Isaac Newton*'s time. He is likewise intreated in the next edition of his dissertations to be a little more careful of his numbers, than he has been p. 134, 135; and to inform himself in spherics; so as to give us the R. Ascens. of the stars truely, from their given longitudes and latitudes.

Lastly the Dr. would inform P. *Souciet*, that the star in the *Centaur*, which *Hipparchus* describes, as being in his time very near the autumnal colure, was not *Bayer*'s Ψ, but certainly his φ; and that in the beginning of the year 1690, its Long. was 8° 43′ 40″ of *Scorpio*, with 27° 59′ S. Lat. But the colure, passing thro' that star, by the proportion given above, cuts the ecliptic 13° 20′ 50″ in antecedence of the star, that is, in 25° 22′ 50″ of *Libra*. But 25° 22′ 50″ give 1827 years: Wherefore, the time this star was in the colure, was 137 years before Christ, when *Hipparchus* flourish'd, and might very well observe it.

A large Stone *voided by a Woman thro' the* Urinary *Passage; by Dr.* Richard Beard. Phil. Transf. N° 397. p. 211.

A Woman in the parish of *Fladbury* in *Worcestershire*, 63 years of age, about three years before, *viz.* in 1724, was afflicted with the usual symptoms of a stone in the kidneys, and afterwards in the bladder. The fits of pain occasioned by it, increas'd as its bulk did; till she was so emaciated by them, that her case was judged desperate. Finding relief, towards the end of summer 1726, by a plentiful use of mallow-tea, she persisted in it for a while: When, on a sudden in the presence of some women, she perceiv'd an uncommon weight and force within, which she assisting

with all the ſtrength and breath ſhe had left, a ſtone came away with a noiſe that very much ſurpris'd the whole company, and with leſs pain and effuſion of blood then, or ſoreneſs afterwards, than might have been expected. She was afterwards eaſy and healthy, and felt no other inconvenience but that unavoidable one, *viz.* an incontinence of urine.

The ſtone (repreſented ſideways in Fig. 2. Plate V. and drawn as near the life as poſſible) was of the ſame colour and texture with others of this kind form'd in human bodies the Dr. ſaw. He found its weight to be two ounces one drachm and 55 grains *avoirdupois*. When firſt voided it was conſiderably more, ſeveral pieces having been rubb'd off at *cc*, and likewiſe on the other ſide: The greateſt circumference is ſeven inches and a half; four inches and three quarters round at the thickeſt place, and four inches $\frac{7}{8}$ long on the convexity; the parts of the ſtone at *aaa* are ſomewhat jagged and ſtain'd with blood, as is the little protuberance on the oppoſite ſide *b*. Here, the Dr. ſuppoſes it met with the greateſt reſiſtance, at the time of its expulſion thro' the urinary paſſage.

Obſervations on a Comet Oct. 1723, *at* Bombay, *together with an* Eclipſe *of the* Moon Oct. 21. 1724, *at* Gomroon *in* Perſia; *by Mr.* William Saunderſon. Phil. Tranſ. N° 397. p. 213.

IN *Oct.* 1723, Mr. *Saunderſon* riding at *Bombay*, a brightneſs appear'd in the heavens in a right line, (or but very little to the eaſtward of one) with *Lyra* and the bright ſtar in the *Eagle*, being about 50° diſtant from the laſt; and on *Monday* the ſeventh following it had advanced 10 degrees towards the *Eagle* moving towards it in the above-mentioned direction, from the S. E. quarter. Mr. *Saunderſon* took the following diſtances between nine and ten o'clock at night.

	Oct. Days.	Diſt. from the *Eagle*'s heart. °	′	
	7	40	00	South.
2	10	23	50	South.
3	11	20	30	South.
4	13	17	40	South.
5	15	14	40	South.
6	19	11	40	South.

At firſt it look'd only like one of the white ſpots, call'd the *magellanic clouds*, the ſpace filling the field of a ſix foot teleſcope: Afterwards he obſerv'd the head in the center of the illuminated ſpace, which did not look very bright; but appeared largeſt on *Oct.* 10, decreaſing gradually both in its bulk and motion from that time till the 25th. When he could find no appearance of it with the above-mentioned teleſcope.

N. B. From the 20th to the 25th it had nearly the ſame place in the heavens, ſeeming to move directly from the earth.

Oct. 21. 1724, Mr. *Saunderſon* being at *Gomroon* in *Perſia*, the moon enter'd into the dark ſhadow of the earth at 11 min. 33 ſeconds paſt 5 *ante meridiem*.

The Propagation of Miſſelto; *by Mr.* Edmund Barrel, Phil. Tranſ. N° 397. p. 215.

THE berries of *Miſſelto* have within their viſcid pulp, a kernel cover'd with a thin whitiſh ſkin; the inner ſubſtance of which is deeply green, and harder than the ſubſtance of a *Piſtachio*-nut kernel: It is flattiſh, and ſhap'd ſometimes like a heart (as Fig. 3. Plate V.) ſometimes oblong (as Fig. 4.) both are as true ſeed, as any plant can have. Thoſe of the oblong form put out but one *germen*; thoſe like a heart two, which prove two diſtinct plants.

Sir *John Colbatch* recommends the ſowing this ſeed by way of inoculation: Accordingly, in *Feb.* 1718-9, Mr. *Barrel* endeavoured to place the berries, within the bark of oak, aſh, beech, pear and apple-trees, by making ſeveral cuts and gaſhes, in the upright ſides of the trees: The whole berries would not ſtay in any of them; and when he broke them, the ſeeds always ſlipp'd out at the edge of the cut, and then it ſtuck to the bark, by means of the ſlimy ſubſtance, with which it is

encompaſſed. He alſo ſtuck one ſeed on the bare bark, without any cutting at all: This ſucceeded beſt, and being of the heart-like ſhape produced two plants. For, about the 28. of *March* 1719, this with two more on the apple-tree, and one on the pear-tree, began to ſhoot; and the growth was in the following manner.

The viſcous matter having ſtuck the ſeed on, and as it dried, drawn the ſeed cloſe and flat down to the bark of the tree, there began in *March* and *April*, to ſpring out of that end of the ſeed, which had been towards the eye of the berry, a ſmall green deep ſhoot, or twig, very like a ſhort piece of a ſmall claſper of the vine. At firſt it aroſe upwards from the bark, and then turning again, as it approached the tree, it ſwelled out ſomewhat bigger round about the end; yet leaving the very tip or bottom quite flat, forming, as it were, a foot to ſtand upon, not unlike the bottom of ſome braſs peſtles. This foot, when it came to the bark, which was about *May* or *June* 1719, fixed itſelf thereon. Being thus faſtened at both ends, it formed a ſmall arch, whoſe diameter was as long as the ſeed, or about $\frac{1}{10}$ of an inch.

In this condition it remained all that year, till about *March* or *April* 1720; and then that part or end of the little ſeedling, which was joined to the bark, at the place where the ſeed firſt ſhot forth, let go its hold, and raiſing itſelf upwards, put forth leaves, and became the head of the plant: And the other end, which ſprung out firſt, and had taken footing in another place, became the root of the plant.

It is no uncommon thing for ſeeds of ever-greens to be two years before they ſpring: And the change of the ends, firſt one of them ſhooting out, and then the other, was what ſurpriſed him moſt at firſt; but on farther reflection he found, that nature is in this plant uniform with its other productions; in carrying the ſap firſt one way to form the root, and then turning the courſe of it back again to ſend out the upper parts of the plant. But what is moſt ſurpriſing is, that the root end ſhould make its firſt ſhoot into the open air, and then turn itſelf down to find a proper place to fix upon. Who could imagine, that a plant, whoſe berry is the moſt orbicular of any; and therefore, the leaſt likely to lie quiet in any ſituation, and whoſe proper place of growth is a round, or wavering bough, or upright ſide of a tree, ſhould after it is once fixed, leave its firſt footing, and ſeek out a new point in the bark to grow upon.

This

This, indeed, is the grand ſecret of the matter, and ſeems to be the very thing that hath kept the world in ignorance, about the growing of this ſeed. For, by requiring a new ſmooth place of the bark, whereon to fix the rooting part, it hath fruſtrated all attempts of ſowing it in the uſual way of other ſeeds.

Theophraſtus (about 2000 years ago) ſeems to endeavour at a reaſon, why this ſeed could not grow in the earth: But all that he, or any one ſince, hath ſaid upon it, is only to agree, that in fact it doth not, and to wonder why ſo perfect a ſeed ſhould not grow in the earth. That ancient author rationally concluded, from its having a ſeed, that the plant muſt come from that ſeed: Whereas latter times have been ſo fond of allowing chance a ſhare in the productions of nature, that *Scaliger* hath not only experimentally confuted the common notion of *Miſſelto*'s being ſown in the dung of the thruſh; but likewiſe argues very ſtrenuouſly, againſt the poſſibility of this plant growing from its ſeed. Even the Great Lord *Bacon*, Sir *Thomas Brown*, *Lobel*, and the inquiſitive Mr. *Ray*, ſo late as 1673, do all give into it, that this plant hath a ſpontaneous, and equivocal, rather than a ſeminal and univocal generation.

Scaliger's ſtrongeſt objection is, to the following purpoſe, '*Miſſelto* ſhoots from branches where it is not poſſible either 'for compoſt or ſeed to ſtand.' *Lobel* objects againſt it, becauſe of the imperfection of the berry, *acinulo illo pallido pellucido*. Mr. *Ray*'s argument is, *viſcus innatus etiam in pronâ ramorum parte*.

If nature had been well examined, it would have appeared, that this ſeed is of a ſubſtance, equal to other kernels; and that the pulp of the berry, with which the ſeed is ſurrounded, is of a more viſcous ſticking nature than the pulp of other berries; for this very purpoſe, that it might be ſufficiently ſtrong to fix the ſeed on any tree, how moveable or upright ſoever the bough or twig ſhould be, whereon it chanced to light.

And, doubtleſs, birds are (tho' not by their dung) ſowers of this, as they are of ſeveral other ſeeds, which they carry away for food; but often drop in places, where otherwiſe they never could come.

Mr. *Barrel* went to gather ſome miſſelto-berries, and found a leaf with a ſeed ſticking thereon, doubtleſs, by a caſual fall out of the bill of ſome bird, that had broken the berry, as ſhe was eating it. There was both a dry ſtring and a dry ſpot of the ſlime upon the leaf, that ſhew how the ſeed was detained there

there in this caſe; and how it may happen in like manner any where elſe.

Mr. *Barrel* ſowed theſe ſeeds on near 30 ſorts of trees and ſhrubs; and yet never had above 10 plants, that held out the ſecond year: So that we need not be ſurpriſed at the little ſucceſs others have had in their trials. This is likewiſe the reaſon, why Mr. *Barrel* could not make many other experiments about the growth of this plant. However ſome caſualties have furniſhed him with two or three, which ſomewhat farther explain the nature of this plant's growing.

1. One of his little plants, ſown in *April* 1724, which was fixed at both ends in its arch-like figure, had in *September* following the middle part broken off; the two ends keeping ſtill faſt to the tree, which ſhews, how firmly the two ends adhere, while it is in that ſtate; and they both continued green for ſome time, and then withered again.

2, That one ſeed, which grew on a pear-tree in 1718-9, was the following ſpring looſened from the tree at one end, as the others were: Yet this ſeedling ſprout never put out any leaves at all, but continued in the ſnme ſtate, neither larger nor ſmaller, near ſix years, that is, till it was broken off by chance in *July* 1725; which ſeems very ſurpriſing: For, a ſeedling plant of any kind is but an embryo, as it were, till it have put forth leaves.

3. His moſt thriving pair of plants of the year 1718-9, being about three inches long, were on the 21. of *May* 1722 ſtruck off, by the falling of a rake-handle againſt them: They took away with them only the outermoſt thin ſkin of the tree; and he could not obſerve any ſigns of deeper rooting. But as he looked now and then on the place, where the miſſelto had grown, he thought he obſerved the bark to ſwell up a little; and on the 12. of *March* 1722-3, he perceived two or three little buds putting forth, and another bud was put forth by the 18. of *March*. They all grew on to have leaves that ſummer; and in *February* 1726-7, they were a cluſter of boughs, of four or five joints in height, and bore berries that winter; whereas two others on the ſame tree, and which were alſo ſown at the ſame time, *viz.* in 1718-9, and were ſix or ſeven joints in height, had not hitherto borne any berries.

The thriving of theſe plants ſo well again, after they were broken off, made him reflect on the *Druids* way of cutting miſſelto from the oak with a gold inſtrument, a metal not apt to take a good edge; and poſſibly the bluntneſs of the inſtrument

ment might be a means to preserve a future growth of the same plant, which, doubtless, they, as well as we, very rarely find to be on the oak. Mr. *Barrel* thinks he might suggest some reasons for this scarcity, from the nature of that bark; and observe several mistakes, into which both modern and ancient writers have run, when they mention this plant; but he only adds this one observation; that there is almost every year, on most misselto-bushes, a visible proof, that the kernel hath a vegetative life in it: For, when the berries hang on till *May* or *June*, the seed will make its little shoot in the berry, as the kernels of lemons, and you may see it coming out at the eye of the berry.

An Account of a Pair of extraordinary large Horns; *by Sir* Hans Sloane. Phil. Transf. N° 397. p. 222.

MR. *Doyley* found a pair of extraordinary large and strangely shaped horns in a cellar, or warehouse at *Wapping*, where they had suffered much by worms and otherwise, being eaten pretty deep on their surfaces in several places. They had lain there so long, that when he bought them, no body could inform him either of the country, whence they came, or when, or how they had been lodged there. In several particulars they resembled the horns of goats, which made many people think, that they had belonged to an animal of that kind, in all likelihood, as large as the mouse-deer in *America* is of its kind. The *Royal Society* being informed of this matter, Mr. *Hunt*, their operator at that time, made a draught of it, on which Dr. *Hook* read a lecture at a meeting of the *Society* at *Gresham-College*. This lecture and the draught are lost: But Sir *Hans Sloane* remembers that Dr. *Hook* suspected them to be the horns of the *Sukotyro*, as the *Chinese* call it, or *Sucotario*, a very large and odd shaped animal, mentioned by *Neuhoff in his Voyages and Travels to the East-Indies* p. 360. *English edit.* and of which he gives a Fig. His description of it is, as follows. 'It is of the bigness of a large ox, with a 'snout like a hog, two long rough ears, and a thick bushy 'tail: The eyes are placed upright in the head, quite diffe'rent from other animals; on the side of the head next to the 'eyes stand two long horns, or rather teeth, not quite so thick, 'as those of the elephant. It feeds upon herbage, and is but 'seldom taken.'

They are both almost straight for a considerable length, and then turning crooked, they run on tapering towards a small and pretty

pretty ſharp point: They are not round, but compreſſed and flattiſh, with large tranſverſe *ſulci*, or furrows on their ſurfaces, waved or undulated on their under parts. They differ ſomewhat in largeneſs. Meaſuring one horn from the large extremity, or baſis A B (Fig. 5. Plate V.) where it was faſtened to the head, along the outer circumference, he found the length A C D to be ſix feet, ſix inches and $\frac{1}{2}$, the length by the line B D was four feet five inches and $\frac{7}{8}$; the diameter of the baſis A B was ſix inches and $\frac{3}{4}$, and its circumference one foot five inches. This weighed 21 pounds, 10 ounces, and contained in the hollow part exactly five quarts of water. In the other horn (Fig. 6.) the length of the outer circumference A C D was ſix feet four inches, the line B D four feet ſeven inches, the diameter of the baſis ſeven inches, and its circumference one foot ſix inches. This weighed 21 pounds 13 ounces and $\frac{1}{2}$, and contained in the hollow part four quarts and a pint; but would have held more, if it had not been very much broken at the large end.

The commander of an *Eaſt-India* merchant ſhip told Sir *Hans Sloane*, that he had ſeen ſuch in the *Indies* on a large *bufalo*'s head. Sir *Hans* is apt to think, that they muſt belong to a very large ſort of bulls or cows, natives of *Æthiopia* and ſome other of the midland parts of *Africa*, and they are mentioned by ſeveral of the ancients; perhaps, not without ſome fabulous additions; tho', what is ſtrange, very few of the modern writers take any notice of them.

Agatharchides a *Cnidian*, who flouriſhed about the CL. *olympiad*, near 200 years before *Chriſt*, is the firſt among the ancients, who mentions and deſcribes this large and voracious bull; and it will appear by what follows, that moſt of the ſubſequent writers have copied him. His deſcription of this animal, in ſome remains of his treatiſe of the *Red ſea*, which are extant in *Photius*'s *Bibliotheca* p. 1364. cap. 39. and from thence printed in the *Geographiæ veteris Scriptores Græci minores*, publiſhed by Dr. *Hudſon*, is, according to the *Latin* tranſlation of *Laurentius Rhodomannus* to the following purpoſe: 'Of the carnivorous bull. It is the fierceſt, and moſt 'untractable of all the bulls I have hitherto mentioned; it is 'carnivorous, much bigger and ſwifter than the common bull, 'and of a remarkably deep yellow colour; his mouth opens to 'his ears, and his grey eyes ſparkle more than a lions; he 'moves his horns in the ſame manner as he does his ears; but 'in fight he makes them ſtand firm; his pile is in an inverted,

'order

'order, contrary to what it is in other animals; he attacks the 'ſtrongeſt wild beaſts, and hunts down all the reſt, and eſpe- 'cially tame cattle; he is the only animal that is invulnerable 'either with lance or bow, which is the reaſon that none can 'ſubdue him, tho' ſeveral have attempted it: It is, therefore, 'rightly thought, even by the *Troglodytæ*, that he has the 'courage of a lion, the ſwiftneſs of a horſe, and the ſtrength of 'a bull; and that he cannot be ſubdued by any iron weapon.'

Diodorus Siculus Biblioth. lib. 3. has barely and almoſt word for word tranſcribed *Agatharchides*, and only added the following particulars; *viz.* that the eyes of this animal are ſhining at night; that after he hath killed other beaſts, he devours them, and that in his attacks on herds of cattle, he is not to be terrified, either by the ſtrength of the ſhepherds, or the great number of dogs. A paſſage relating to this animal in *Strabo Geogr. lib.* 16. p. 775. *edit. Caſaubon* is to the following purpoſe; 'there are likewiſe in *Arabia* wild bulls, that are 'carnivorous, and that far ſurpaſs our bulls, both in bigneſs and 'ſwiftneſs, and of a deep yellow colour.' *Pliny Hiſt. Nat. lib.* 8. *cap.* 21. ſeems likewiſe to have copied *Agatharchides*, and ſpeaks of them to the following purpoſe; 'but there are 'very fierce wild bulls in *Ethiopia*, larger than the tame ones 'and ſwifter than any other animal, of a deep yellow colour, 'with blue eyes, and their pile inverted, with a mouth that opens 'to their ears, and moveable horns; their ſkin is as hard as a 'flint, and invulnerable; they hunt down all other wild 'beaſts; and they are caught in pits.' In the 45th chapter of the ſaid 8th book of *Pliny's nat. hiſt.* he mentions a ſort of *Indian* oxen, 'tall as camels, and whoſe horns are four foot 'broad.' It is not unlikely, but that theſe *Indian* oxen are the ſame with the *Ethiopian* ones above-deſcribed; eſpecially if we ſuppoſe. that the tranſcribers of *Pliny* have by miſtake written *latitudinem* inſtead of *altitudinem*. *Solinus Polyhiſt. cap.* 52. *p.* 58. *edit. Salm.* has barely copied *Pliny*, only with this difference, that he calls them *Indicos tauros*; whereas *Pliny* himſelf has deſcribed them amongſt the *Ethiopian* beaſts, which might very well happen, *Ethiopia* being reckoned by ſome of the ancients, as part of *India*. *Ælian*'s deſcription *hiſt. animal. lib.* 16. *cap.* 4. perfectly agrees with that of *Agatharchides*, of whom it ſeems, he alſo borrowed it; only he fixes the ſize of theſe extraordinary oxen to twice the bigneſs of the *Grecian* ox. There is another paſſage in *Ælian hiſt. animal. lib.* 3. *cap.* 34. which ſeems to relate, both

to

to this large kind of *Æthiopian* oxen, and to the horns in Sir *Hans Sloane*'s possession, and is to the following purpose, 'they 'report, that a horn was brought *Ptolemy* II. out of *India*, 'that would contain 3 *amphoræ* or 27 gallons: Whence we 'may conclude the hugeness of the ox.' *Ludolfus* in his *hist. Æthiopica lib.* 1. *cap.* 10. speaking of the large *Ethiopian* oxen, conjectures, that they are the *taur-elephantes*, which *Philostorgius*, a *Cappadocian*, *lib.* 3. *cap.* 11. says were brought to *Constantinople* in his time, where he saw them. The words of *Philostorgius*, as transcribed by *Ludolfus* in his *Comment. ad hist. suam Æthiop. p.* 145. are to the following purpose, 'in 'that country there are huge elephants, nay *taur-elephants* as 'they are called, which in all other particulars is a huge kind 'of ox, but in skin and colour an elephant, and almost of the 'same bigness.'

From all these writers it appears, that there is in *Ethiopia* (and probably in the midland parts of *Afric*, where travellers seldom come) a very large animal of the ox-kind, at least twice as big as our bulls or oxen, with horns proportionably large, but otherwise differing from them in several respects. And it is confirmed by modern writers, that there is such an animal in those countries; tho' there be none, Sir *Hans Sloane* knows of, that hath given the least satisfactory description of it. *Ludolfus* in his *hist. Æthiop. lib.* 1. *cap.* 10. affirms, that there are in *Ethiopia* bulls of an uncommon size, twice as large as those in *Hungary* and *Russia*; and that having shewn some of the largest oxen in *Germany* to *Gregory*, an *Abyssinian* (from whose writings, and informations, he collected the materials for that work) he said, they were but of a middling size. The letters of the *Jesuits* frequently mention the largeness of these oxen, and the said *Ludolfus* cites the following passage out of a letter of *Alphonsus Mendezius*, patriarch of *Ethiopia*, dated *June* 1. 1626, which is to the following purpose, 'here 'are very large oxen, with such exceeding thick and long 'horns, as that one of them would contain a small uter of wine.' F. *Bernier* in his account of the *Great Mogul*'s country *Tom.* 2. *p.* 43. affirms, that among several presents, which two *Ethiopian* ambassadors presented to *Aurengzeb*, there was a prodigious large horn of a bull, full of civet, which he measured and found the basis, or large end ½ a foot in diameter.

Upon the whole, it appears to Sir *Hans Sloane*, that these horns, and likewise that mentioned by *Bernier*, are the horns of a large sort of bull or cow in *Ethiopia* and the inland parts

parts of *Africa*; which, in all likelihood, is the ſame with that deſcribed by *Agatharchides*, *Pliny*, and thoſe other ancient authors abovementioned. But hitherto he could not, for want of a more accurate deſcription, be certain, whether it be the ſame with the *Sukotorio* or *Sukotyro* of *Neuhoff loc. ſupra cit.* tho' there be a good deal of reaſon to think that it is.

Geſner Icon. Animal. quadrup. edit. 2d. Tigur. 1560. *p.* 34. ſpeaks of a very large horn, which was hung by a chain to a pillar, in the *Minſter* or cathedral of *Strasburgh*, and which is not unlikely to be of the ſame ſort with theſe. He ſays, that its outer circumference meaſured four *Roman* yards in length, and he conjectures it to have been the horn of a large old *urus*, which was hung up there for its prodigious ſize, 2 or 300 years, probably, before his time. As to the horns in Sir *Hans Sloane*'s poſſeſſion, it is very likely, that when the *Engliſh* had a great commerce at *Ormus*, they were brought thither from ſome neighbouring country, and afterwards carried over into *England*, by ſome curious perſon.

The Anatomy of the Mus Alpinus *or* Marmot; *by Dr.* John James Scheuchzer. Phil. Tranſ. N° 397. p. 237. *Tranſlated from the* Latin,

UPON opening the *abdomen*, there immediately preſented to view a very fat *omentum*, whoſe fatty parts, interwoven with the *epiploic* blood-veſſels, form a very beautiful network; but the fat of this *omentum* is more concreted and compact than that to be deſcribed anon, and which was emaciated, as it were; ſo that one may conclude that the more fluid parts of this oily liquor (during the animal's ſleeping all the winter) were abſorbed by the *vena portæ*, in order to ſerve both for ſecreting the bile, with which the gall-bladder was very turgid, and for nouriſhing the body itſelf.

On both ſides of the *hypogaſtrium* there preſented to view a very large quantity of fat, of a laxer conſiſtence than that of the *omentum*; which thence extended itſelf from the kidneys to the groin, forming another, as it were, nay a double *omentum*; which fat, as that of the meſentery, that accompanied all the inteſtines, ſerves both for lubricating the abdominal *viſcera*, and for a plentiful repoſitory of nutritious juice.

The *pancreas*, and that very much emaciated, extended itſelf to the ſide of the *duodenum*, as uſual, nay beyond its curvature; in this emaciated ſtate Dr. *Scheuchzer* obſerved all the other glands of this ſubject, eſpecially ſuch as lay among the

 muſcles:

muſcles: In theſe animals the circulation of the blood and all the ſecretions are exceeding ſlow; there is no return of *ſerum* or *lympha*, of which there is, it is true, a ſlow, but yet a ſucceſſive ſecretion; ſo that at length the blood is almoſt deprived of its *ſerum*: Hence it is not ſupriſing, that for want of *lympha* to be ſecreted, the *pancreatic* gland, and conſequently the other glands were emaciated.

Upon opening the *duodenum*, there preſented to view a frothy bile, which very thing ſhews want of *ſerum*.

The *cornua uteri* extended themſelves two *Paris* inches; the *tubæ*, ſcarce thicker than a thread, to ½ an inch; the *ovaria* were about two lines in length and one line in breadth, white, but, when viewed with a microſcope, pellucid, with prominent *ovula* diſtinctly tranſparent.

The liver, which was pretty large, had ſix lobes, ſome of which were ſubdivided into two by inciſure; the lowermoſt lobe was connected by an intermediate membrane with the right kidney.

The kidneys were entirely ſurrounded with fat.

The *renes ſuccenturiati* were ſmall yellowiſh bodies at the ſide of the *vena cava* above the emulgents, about three lines in length and ſcarce ½ a line in breadth, plainly appearing between the liver and kidneys, but emaciated, as were the other glands.

The ſtructure of the ſtomach, as in other prohibited and carnivorous animals, was ſimply membranous, tho' this animal be graminivorous.

The inteſtinal tube in the confines of the thin and thick guts was of a peculiar ſtructure.

The entry of the *ileon* into the *colon* was only three lines in diameter; the *colon* over againſt the *ileon* two lines; but the *cæcum*, incurvated in that place, was two inches in diameter: The valve of the *colon*, as it is called, was remarkable, being round like a ring, and of a very peculiar ſtructure; for, it was very ſlender.

The entry into the *ileum* was between two *valvulæ conniventes*; ſo that the exerements could have no regreſs into the thin guts; and the two membranes, which by their mutual concourſe form that valve, are of a rhomboidal figure; beſides, there were other annular *valvulæ conniventes*, extending into three or four principal branches, as it were, towards the *cæcum*. This obſervation does very much illuſtrate the uſe of the *cæcum*, which in new-born children is commonly larger; it ſerving as a *diver-*

diverticulum to the excrements, that for the nine months of gestation are usually collected in the guts, and not discharged. The like holds in animals, that, during the winter, sleep in the caverns of mountains: There is no discharge of the *fœces* all that time; and notwithstanding the very slow circulation and secretion, and the taking no food all the time, they are collected; and lest the thick and thin guts should be over stuffed, they are conveyed to the *cœcum*, where they remain till the spring; the regress of the *fœces* from the *cæcum* into the *colon* is especially hindered by the above-described valves.

As to the myography, the Dr. especially considers, what distinguishes the actions of this animal from those of others: Immediately under the *cutis*, is a thick and strong muscle, call'd the *platysma myoides*, that involves the interior and lateral parts of the neck, nay, the whole *masseter* like a swathe; and extending to the articulation of the *humerus* with the *cubitus*, is inserted both into the lower and upper lip, and considerably assists the frequent flexory motion of the fore-feet and lips: For, these animals, laying hold of their food with their fore-feet, instead of hands, bring it to their mouth; and they also dig the ground with them.

The thyroidal glands at the sides of the *larynx* are large; undoubtedly, to moisten the neighbouring muscles which are frequently employed; for which very purpose, other glandulous bodies lie here and there among other muscles.

The *sternohyoideus* and *sternothyroideus* were in their usual situation.

The *masseter* was exceeding strong and tendinous, of very great use in these animals.

The *digastricus*, the depressor of the lower jaw, was likewise very strong, which arising from the basis of the *os hyoides* and its *cornua*, as also by a strong tendon from the *processus styloides*, is inserted into the lower jaw almost as far as the chin; its anterior belly is especially thick, and four times stronger than the posterior.

Instead of the *geniohyoideus externus*, there is immediately under the chin a muscular, thick, fleshy mass, of a triangular figure, from the angle of the lower jaw, extending laterally into five lines, which principally seems to connect the inferior jaw: Next to this the *musculus latus* lies upon the basis of the tongue, arising with transverse fleshy fibres from the *parietes* of the lower jaw; and then is inserted into the middle of the base of the

the tongue, and afterwards into the *os hyoides*; and which seems to supply the *geniohyoideus externus*; it is thin, and can scarce be separated from the *geniohyoideus internus* or *genioglossus*, on which it lies.

The *styloglossus* is likewise a strong muscle, and arising with a fleshy beginning from the styloid process, terminates in a strong tendon.

The *cricothyroid*, and likewise the *thyrohyoid* muscles appear very plainly.

On the sides of the *gula* and *aspera arteria* there are glands extended on both sides, almost the length of an inch; which do not appear but upon raising the *aspera arteria* and *gula*, and seem to serve for lubricating these parts.

The *musculus rectus anticus major* is strong, and entirely of the same structure, as in a human subject.

The *musculus ceratohyoides triangularis*, arising from the *cornua* of the *os hyoides*, is inserted into its basis.

The *thyroarytænoideus* plainly appears under the sphincter of the *gula* and especially serves for forming the peculiar cry of these animals, by strongly constringing the *arytænoid* cartilages.

The *musculi pterygoidei* are very strong.

As to the above-described *platysma myoides* it is farther to be noted, that a pretty thick portion of it is inserted into the spine of the *scapula*, and that another thin muscle, lying on the *trapezius*, is transversely inserted into the said *platysma myoides*, which then uniting from several parts into one muscle, serves to move the lips, bend the *cubitus*, raise the *scapula* and draw the arms forwards, by means of which these animals dig the ground, and perform other functions.

The *trapezius* is entirely the same as in a human subject.

Instead of the *rhomboides*, a strong muscle under the *trapezius* presents to view, which arises not only from the superior spines of the *vertebræ* of the back and the inferior ones of the neck, but from all the transverse spines of the *vertebræ* of the neck; and besides, from the *occiput*; so as not only to move the *scapula* backwards, but likewise raise it, and so assist the *musculus patientiæ* in its action.

This insertion of the *rhomboides* is likewise observ'd in other quadrupeds, and undoubtedly it serves for this purpose, namely, the better to sustain the head hanging in a prone posture.

The *musculus patientiæ* arises with a small beginning from the transverse *apophysis* of the first *vertebræ*, and is inserted into the beginning of the spine of the *scapula*.

Another muscle, arising from the same beginning, assists the *musculus patientiæ* in its action, and is inserted into the termination of the said spine; both which together raise the whole *scapula*.

Between the *serratus anticus major* (which is the same as in a human subject) and the *subscapularis* there is a large and broad glandulous mass of flesh, which serves to lubricate these muscles.

The *serratus anticus major* has not only indented beginnings from the ribs, but likewise strong and thick originations from the transverse processes of the *vertebræ* of the neck.

There is a double *aniscalptor*, one above the other, the inferior, being the broader and stronger muscle, arises by digitations, from the ribs, especially the lower ones, and in its passage, being strongly connected with the inferior *costa* of the *scapula*, is inserted by a pretty broad basis into the *humerus*, as in human subjects; and besides, it sends a small slip to the *olecranon*; so that it not only serves to draw the *humerus* backwards and downwards, but likewise to extend the *cubitus*.

The muscles of the *humerus*, *viz.* the *infraspinatus*, *subscapularis*, and both the *rotundi*, are the same as in a human subject.

There is a double *deltoides*: That part, which arises from the clavicule and *acromion*, is inserted into the flexure of the *cubitus*; and consequently, belongs not so much to the muscles of the *humerus*, as to those of the *cubitus*; the other part, which arises from the *acromion* and the spine of the *scapula*, is situated on the outer part of the *humerus* and inserted about the middle thereof: and therefore it not only moves the *humerus* upwards, but draws it outwards.

The *coracobrachiæus* is a small muscle, having the same situation and use, as in men.

The first of the *extensores cubiti* is a slender muscle, covering the rest like a swathe; it arises from the *costa* of the *scapula*; two other strong muscles follow, one of which likewise arises from the *costa* of the *scapula*, and the other from the superior part of the *humerus*; and as they go on for some-way, they unite together, and are inserted by a strong tendon into the *olecranon*.

Under

Under these there lies another strong muscle, which arising from the uppermost part of the *humerus* unites with the *anconæus*.

This is stronger and thicker than in men, and possesses the outer and lower side of the *humerus*.

Among the *flexores cubiti*, the *biceps* has the same structure, as in men.

The origin of the *brachialis internus* extends to the head of the *humerus*; and this muscle in its progress encompasses the whole external part of the *humerus* like a swathe, and is inserted in the same place as in human subjects.

There are three *flexores carpi*; the *extensores* are the same as in men.

The *extensores digitorum* are first the *communis*, which, arising from the *humerus*, is inserted into the three first fingers; the second descending from the *cubitus*, is inserted into the last and last but one.

In this subject there is the *indicator*, or *extensor indicis*.

The *pronatores* are the same as in human subjects.

Of the *supinatores* the *longus* is shorter than the *brevis*; but its situation the same as in men.

A B (Fig. 7. Plate V.) represents the *gula*; C D the *duodenum*; E the stomach.

E H (Fig. 8.) represents a portion of the *ileon*; G H a portion of the *colon*; H F I the *cœcum*; all as big as the life.

a a (Fig. 9.) represents the valve of the *colon*, almost the same as delineated in human subjects, only that it is nearly *rhomboidal*; *b c* the aperture of the *ileum* into the *colon*.

K (Fig. 10) represents a portion of the *cœcum*, whose lowest part is open towards the *colon*, that the *valvulæ conniventes* o o o may be observed.

An Aurora Borealis *Oct.* 8. 1726, *together with an Account of the Cause of the* Phenomena; *by Mr.* Derham. Phil. Transf. N° 398. p. 245.

MR. *Derham* observed two sorts of streamings; one by way of explosion from the horizon; the other by opening and shutting, without shootings up or swift dartings.

Of the latter sort chiefly, was that of *Oct.* 8. 1726, in which tho' the streams, spires, lances, cones, or whatever else they may be call'd, were as large and remarkable, as in that in the year 1715-6; yet they exhibited themselves principally by the

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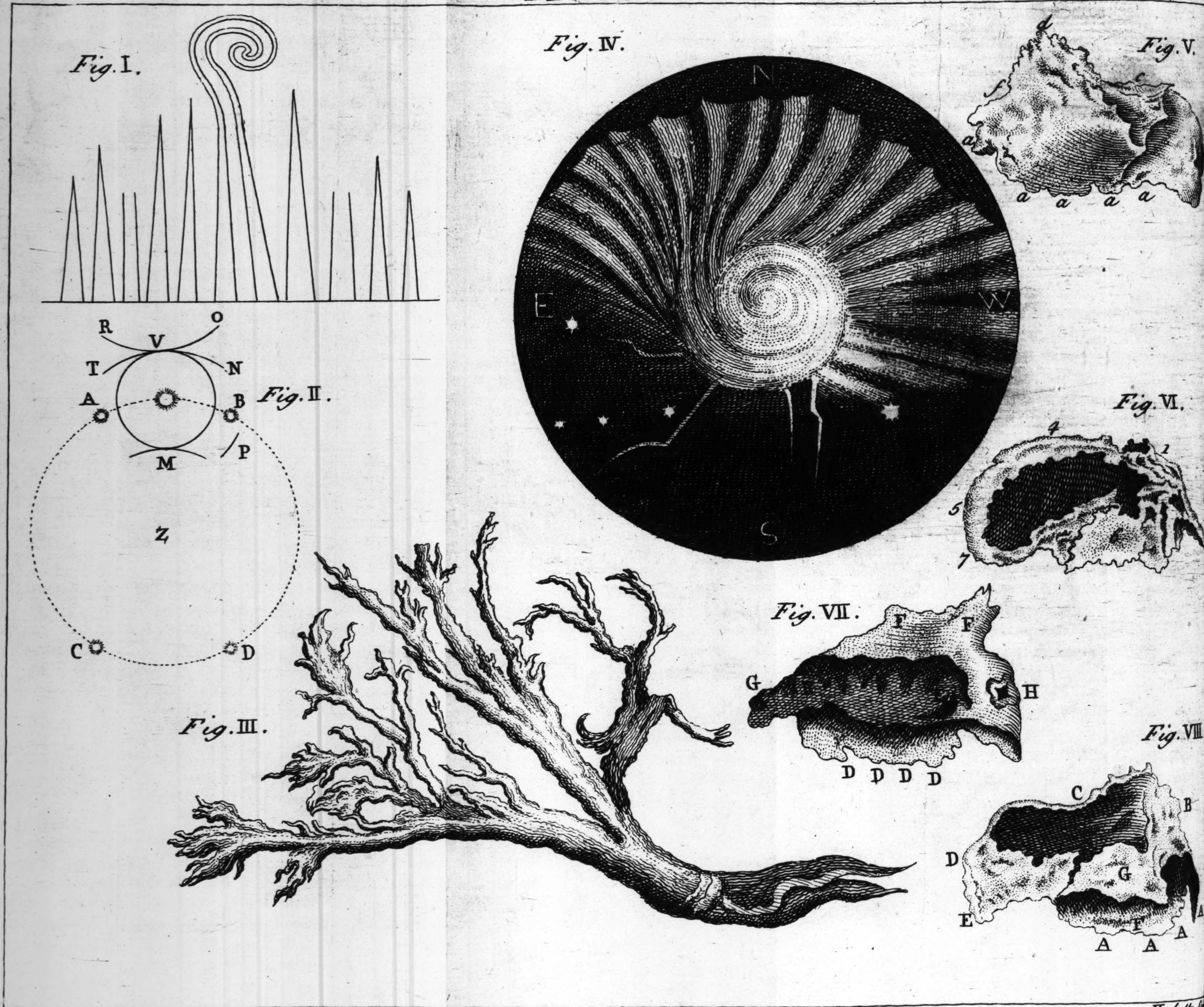

G. Smith del. Hulett sc.

vaporous matter opening and ſhutting, as if a curtain had been drawn and withdrawn before them.

The firſt view he had of this phenomenon was exactly at 8 o'clock in the evening: At which time all he obſerv'd was a long narrow *faſcia*, reſembling a white ragged cloud, extended croſs the heavens, from W. b. S. to E. b. N. which in a few minutes began to emit ſome ſtreams, and then diſappear'd; which was ſucceeded by a deal of ſtreaming in the northerly parts; and in a quarter of an hour it likewiſe began to reach other points; and ſoon after that it ſtream'd all round, in the ſoutherly, eaſterly, and weſterly parts as much, or nearly as much as in the north; which was a thing he never obſerv'd before in theſe phenomena.

Theſe ſtreams, or cones were for the moſt part pointed (Fig. 1. Plate VI.) ſo as to make the appearance of flaming ſpires, or pyramids; and ſome others were truncated, and reached but half way; ſome alſo were longer, and ſome ſhorter; others of them had their points reaching up the zenith, or near it, where they formed a ſort of canopy, or thin cloud, ſometimes red, ſometimes browniſh, ſometimes blazing, as if fired, and ſometimes emitting ſtreams all round it, which at that time gave it the appearance of ſuch a ſtar, as our knights of the garter wear on their breaſts.

This canopy was manifeſtly formed by the matter carried up by the ſtreaming on all parts of the horizon; which matter ſometimes ſeemed to aſcend with ſome force, as if impelled by the *impetus* of ſome exploſive agent below, as has been ſaid happened in the ſtreaming of *March* 1715-6, of which Mr. *Derham* gave a large and particular account in a former *Tranſaction*. This forcible aſcent of the ſtreaming matter gave a motion to the canopy, ſometimes a gyration, like that of a whirlwind; which was manifeſtly cauſed by the ſtreams ſtriking the outſide parts of the canopy, as in the Fig. But when the ſtreaming matter hit the canopy in the middle, all was then in confuſion: The canopy or *corona* did not continue in one place, but ſhifted its poſition; ſometimes higher near the zenith, and then towards the eaſt and ſouth-eaſt 10 or 15 degrees; and then back again nearer the zenith, according as the darting matter directed it; but he does not remember that this canopy was at any time directed towards the weſtern points.

Theſe two particulars, namely, the ſtreaming all round in all parts of the horizon, aud the canopy in and near the zenith, are what were taken notice of in all parts of *England*,

Mr. *Derham* met with any accounts from; particularly in *Northamptonshire*, *Staffordshire*, *Oxfordshire*, *Wiltshire*, *Berkshire*, *Middlesex*, *Somersetshire* and *Essex*; and in divers parts beyond sea.

Mr. *Wasse* gave Mr. *Derham* the following account of its appearance at *Aynho* in *Northamptonshire*, that at 7^h $20'$ *p.m.* he observed an arch somewhat incurvated, at first resembling a rainbow and about half its breadth, and of a yellow colour; which in about 10 min. began to twist, and form an angle at the zenith; that one end of it was pretty much to the east, and not directly to the north; and the western end deflected as much to the south; that it remained after the twist, at the zenith, without any considerable motion, not a quarter of an hour: After which the rods arose on all sides, from the horizon to the zenith, the upper points seeming to move thro' a sort of vortex quite out of our atmosphere: Which rods he thinks, rose perpendicularly from the horizon; but seemed to converge towards the zenith, according to the rules of perspective, by their angle being then less than their basis at the horizon; that a redness was perceived, which he thinks was strongest towards the west; which colour did not appear, till the arch broke into several pieces, and overspread the heavens with a thin faintish fire, thro' which they observed *Jupiter* very clearly.

This account of Mr. *Wasse*'s may shew, how the phenomenon was in *England*; because most of the accounts Mr. *Derham* met with agree in the main with his. But in the more southerly parts of *Europe*, Mr. *Derham* takes it to have been somewhat different.

The news papers tell us from *Schaffhausen*, 'that on the '19. of *October* there was a great alarm in several parts of '*Switzerland*, on account of a great light, observed in the 'air, from 7 o'clock in the evening till midnight; which 'was supposed to be the reflection of some great conflagra-'tion. At *Bern* every body thought there was a fire in some 'part or other of the city or neighbourhood. At *Neufchatel* 'the alarm-bells were rung, and the Governour several 'hours on horse-back, to give orders, &c. as in cases of 'distress. All which they heard afterwards was only an *au-'rora borealis*.'

And from *Florence*, Sir *Tho. Derham* sent him the following account. 'As to the *lumen boreale* which appeared in 'these parts on *October* 8. I observed it myself in the follow-'ing

'ing manner: It was an hour and a half after ſun-ſet, when 'I was paſſing thro' a piazza in this town, that I diſcovered 'the phenomenon, which ſeemed a mile long and three 'quarters of a mile broad, an almoſt perfect oval figure, 'hanging north and north-eaſt to us: The edges of it were 'of a pale light colour, like the firſt dawn of the morning; 'and towards the center, it increaſed its fiery colour: So 'that in ſome places it looked like the fire of a furnace; 'but in the very center, and ſeveral adjoining parts, it re-'ſembled a red-hot iron growing cold, that ſeems bloody. 'For a good while I could obſerve no motion in it; but after '¼ of an hour, I diſcovered a general ſlow motion back-'wards and forwards, like what one (by the help of a mi-'croſcope) ſees of the circulation of the blood in the tails of 'fiſh, but no manner of darting: Inſomuch that in another '¼ of an hour it vaniſhed imperceptibly, juſt like a rain-'bow; and the air grew dark again, that before was ſo lumi-'nous, that one might read a manuſcript by it. It is very 'remarkable, that at *Fieſole*, a town within a ſhort mile of '*Florence*, the phenomenon ſeemed to thoſe inhabitants to be 'between them and us, and they thought our town was 'burning: Whereby it appears not to have been very deep, 'nor very high; *Fieſole* ſtanding upon a hill half a mile 'high, and to the north-north-eaſt of *Florence*.'

To theſe obſervations Mr. *Derham* adds two or three things more.

One thing, that was taken notice of in moſt places, was, that in ſome part of the largeſt ſtreaming, the vapours between the ſpires or lances were of a blood-red colour: What he himſelf obſerved was, that about ½ an hour after 8 o'clock, the vapours towards the ſouth-weſt were very denſe, and for ſome time red; and not long after, the like redneſs aroſe in the north-eaſt, and the other gradually went off: Both which gave thoſe parts of the atmoſphere the appearance of blazing lances, and blood-coloured pillars.

Another thing Mr. *Derham* took notice of was a ſtrange commotion and working among the ſtreams as if ſome large cloud, or other body was moving behind, and diſturbed them.

In the northerly and ſoutherly parts the ſtreams were perpendicular to the horizon; but in the intermediate points they ſeemed to decline more or leſs one way or other; or rather to incline towards the meridian.

As to the weather, the preceeding day was cloudy, with an hoar-frost in the morning; but it cleared up, and became warmer afterwards; but towards the horizon very vaporous. And the next morning (after the streaming) before sun-rising, the air was full of vapours, with divers thin vaporous clouds, some of a bright brown, and others reddish, which he took to be remains of the streaming, which, as he was informed, continued all night.

As to the cause of these phenomena, Mr. *Derham* takes it to be from the same matter, or vapours, which produce earthquakes; and that for the following reasons.

1. Because some of these phenomena have been followed by earthquakes; as that which *Stow* gives an account of in his *annals* on *Nov.* 14. 1574, in which he says, 'were seen in the 'air strange impressions of fire and smoke to proceed forth of a 'black cloud in the north towards the south; that the next 'night following, the heavens from all parts did seem to burn 'marvelous ragingly, and over our heads the flames from the 'horizon round about rising did meet, and there double and 'roll one in another, as if it had been in a clear furnace.'

And after this (he tells us) 'followed on the 26. of *February* 'great earthquakes in the cities of *York*, *Worcester*, *Gloucester*, '*Bristol*, *Hereford*, and in the countries about, which caused 'the people to run out of their houses, for fear they should 'have fallen on their heads. In *Tewksbury*, *Breedon*, &c. 'the dishes fell from the cupboards, and the books in mens 'studies from the shelves:' With more to the same purpose.

So this phenomenon in *October* 8. 1726. was preceeded by that fatal earthquake at *Palermo* in *Sicily*, and succeeded by one in *England*, on *Tuesday October* 25. following. This Mr. *Derham* was informed, was perceived at *London*; and was very considerable at *Dorchester*, *Weymouth*, *Portland*, *Portsmouth*, *Purbeck*, and divers other places in *Dorsetshire*, that it caused the doors to fly open, shook down pewter off the shelves, and was felt in some ships that lay in the harbours.

2. Another reason is, that Mr. *Derham* was assured by an ingenious sensible Gentleman of his acquaintance, that as he was viewing this appearance, on the top of his house at *Little Chelsea*, he plainly perceived a sulphureous smell in the air; and that another person did the same, on the top of another house near him.

3. Another thing, which concurs with what has been said, is, that Mr. *Derham* was assured by several persons, that an hissing,

hiſſing, and in ſome places a crackling noiſe was heard in the time of the ſtreaming, like what is reported to be often heard in earthquakes.

And now to conclude, Mr. *Derham* remarks two things on what has been ſaid.

1. That it may help the ſagacious meteorologiſt to reſolve ſeveral difficulties relating to theſe northern lights, to obſerve, that what was ſtreaming, or darting in our northern parts, was only a remarkable light or blaze in *Italy*, and the ſouthern parts; if he takes Sir *Tho. Derham*'s and the news-papers account right.

2. If thoſe phenomena have the ſame origin that earthquakes have, that then they are, undoubtedly, of great uſe to the peace and ſafety of the earth, by venting ſome of that pernicious vapour and ferment that is the cauſe of thoſe terrible convulſions, which earthquakes are accompanied with.

In moſt of the *northern lights* Mr. *Derham* obſerved, there generally was a dark bank of vapours, circular at top: But whether this of *October* 8. had any ſuch arch, he could not ſee at *Upminſter*, where he was ſurrounded with trees.

An Account of the ſame Phenomenon *at* Southwick *in* Northamptonſhire; *by Mr.* George Lynn. Phil. Tranſ. N° 398. p. 253.

Oct. 8. at 10 o'clock in the morning	Alt. of barometer 29.90	Thermometer 54.	Wind W. 1.	weather fair and clear

This evening there appeared an *aurora borealis*, Mr. *Lynn* thinks, full as remarkable as that in *March* 1715-6, tho' varying in form: It began about 6 o'clock in the evening to be light in the north, with ſtreaks proceeding from it, and it gradually ſpread both towards the eaſt and weſt, the ſouth being ſtill very clear; but before 7 it left all the northern parts (excepting towards the zenith) and covered all the ſouthern parts: Soon after which there appeared a white arch proceeding from eaſt to weſt, paſſing near the zenith, but more to the ſouth, which ſeemed fixed for a time; but about 10 minutes after 7 it was diſperſed and immediately ſucceeded by a kind of *glory* of an oval form, the longer axis from eaſt to weſt was ſomething ſouth of the zenith, with rays ſhooting up from all parts, and interchanging ſwiftly, for about 15 or 20 degrees from it; the reſt of the heavens (excepting the north, which ſtill continued very

very clear) afforded various phenomena. In the east there was a quick succession of columns of the rain-bow colours inclinable to white, the west to a purple; and there appeared about the south-west for a considerable way a blood-red coruscation, which continued 5 or 6 minutes.

In a quarter of an hour these appearances became less remarkable; tho' the *aurora* continued for most part of the night, and afforded a light generally equal to that of the moon in her quadratures. Looking with his telescope at *Jupiter*, he found both his satellites and his belts appear as plain thro' the *aurora*, as if the sky had been perfectly clear.

A Register *of Observations of the* Aurora Borealis *for 4 Years at* Lynn; *by Mr.* Rastrick. Phil. Trans. N° 398. p. 255. *Translated from the* Lation.

ON *Tuesday Feb.* 19. 1722-3 the *aurora borealis* appeared at ½ an hour after 10 o'clock in the evening.

On *Friday March* 15. 1722-3 from 8 o'clock in the evening till midnight.

On *Tuesday Aug.* 20. 1723.

On *Sunday Oct.* 20. 1723 from 6 o'clock in the evening till midnight.

On *Sunday Sept.* 26. 1725. an *aurora borealis* appeared from 7 till 10 o'clock in the evening, with a variety of radiations.

On *Monday Oct.* 3. 1726, all night long.

As also on *Tuesday Oct.* 4. 1726.

As also on *Saturday Oct.* 8. 1726. A very surprising phenomenon, which is very accurately described by Drs. *Langwith*, *Huxham* and *Hallet*, as also by Mr. *Hadley* and M. *Calandrini* in *Phil. Trans.* N° 395.

On *Wednesday Oct.* 26. 1726, about 10 o'clock in the evening.

On *Friday Mar.* 3. 1726-7 from 8 o'clock in the evening till midnight. The *aurora* this night (as it appeared to Mr. *Rastrick*) was much more surprising than that of *Oct.* 8. and he thinks, it nowise differed (according to the descriptions given of it) from that remarkable phenomenon of *March* 6. 1715-6.

On *Sunday March* 5. 1726-7.

Four Parhelia *observed at* Kensington; *by Mr.* George Whiston. Phil. Transf. N° 398. p. 257.

On *Wednesday March* 1. 1726-7 walking in a garden at *Kensington* about ¼ after 10 o'clock, Mr. *Whiston* happened to observe the following appearance.

He at first took notice of the halo about the sun V M (Fig. 2. Plate VI.) with its usual circumstances, which are pretty frequent; its upper part was very luminous, with a confused mixture of the rainbow colours in it, and it was touched at the vertex with the two other curvatures O V R, N V T, in the situation as represented in the Fig. tho' the latter arch N V T did not appear till some time after: The bottom part of it at M, which appeared a little above the horizon, had likewise something of the same nature, but not in so great a degree.

He likewise immediately perceived the two parhelia A, B, whose diameters were pretty large, and whose brightness and colour were pretty much the same with the upper part of the halo.

As the halo was not at that time quite perfect, but had some parts interrupted, he thought that the two parhelia were in the circumference of its circle, as usual; but after about ¼ of an hour, he directly observed the halo to pass between the parhelion A, and the true sun; and he had no reason to doubt the same of the other B; tho' he do not remember, that he directly observed that.

The *parhelia* A, B, therefore, which were but a little distant from the circumference of the halo, began now to appear with narrow, pale, whitish streaks of light, in the nature of tails, proceeding from them; but they soon extended themselves so far, that they met in the point opposite to the sun, and formed the great circle A B C D, parallel to the horizon, whose breadth was about ½ that of the halo.

Upon viewing it carefully all round, he soon discovered a third mock-sun C, of a plain whitish colour, without any mixture (which was also the case of the great circle) and immediately also a fourth D, both these pretty exactly resembling each other (as the two first did likewise each other) very much inferior to the *parhelia* A, B, in brightness, tho' not so much in magnitude; for, he estimates their diameters to have been to the two first *parhelia*, as 4 to 5.

As he had no opportunity of measuring the several angles, he has placed the mock-suns C, D in the Fig. rather in agreement with

with former obſervations, than his own conjectures; for, they appeared to be at a greater diſtance from each other, and nearer reſpectively to the two firſt *parhelia*; which difference M. *Huygens* attributes to the different altitude of the ſun.

The arch N V T, not being very viſible while the great circle was; and indeed not extending itſelf at any time near ſo far, as to the *parhelia*, or to the circumference of the great circle, he could not determine by a direct obſervation, whether the *parhelia* A B appeared in the interſection of that circle, produced with the great circle; but the curvature appeared to him ſo plainly different from that, its center not being, he reckons, above M, that he is apt to think, the *parhelia* A, B, were neither in the interſection of N V O with the circle A B C D, nor of the halo with the ſame circle; in one of which circumſtances they have hitherto appeared; but between thoſe two points, and much nearer to the circumference of the halo.

He likewiſe thought he plainly ſaw at one time a ſmall portion of a ſecondary halo, if it may be ſo called as at P in the Fig. It evidently appeared to be an arch of a circle, concentrical with the halo, and tinged with the rainbow colours; whoſe diameters might perhaps be to that of the halo, as 4 to 3: But as it appeared but for a little time, he would not be poſitive about it.

He does not at all remember, that during the time he watched it, he ever obſerved the great circle A B C D, to be viſible within the halo, between A and B; tho' all the other parts of it were ſometimes very perfect.

This face of the heavens continued, tho' with an interruption of ſome parts at intervals, till about $\frac{1}{4}$ after 11 o'clock; when he left it, and could not return till about 12; at which time the ſky, which had before been only hazy (a ſure criterion of theſe appearances) was overcaſt with clouds, aud this phenomenon was no longer viſible.

A Deſcription of ſome rare Cryſtals *newly diſcovered; by Dr.* John James Scheuchzer. Phil. Tranſ. N° 398. p. 260. *Tranſlated from the* Latin.

ON the high cliffs of *Grimſul* there was a few years before diſcovered in the middle of the rock a vein of cryſtals, which is now exhauſted; than which, probably, there never was ſeen any larger, or purer. The greateſt part of them, amounting to about 60 centners, was to be ſold, but at an extraordinary price, as appears from what follows; but beſides their purity, their

their uncommon ſize may excuſe ſuch a price. The following is a ſeries of the chief cryſtals, as Dr. *Scheuchzer* had it from M. *Friſching*, in whoſe cuſtody they were.

N° 1. of 2 centners or 2 centners and ½, is two feet nine inches and a half long; three feet ſeven inches and a half round; very clear, and hexagonal: Valued at one *Louis d'or* and a half, the pound.

N° 2. of 136 *lb.* is two feet three inches long, two feet nine inches round, with ſome purple ſpots on the edge, very pure in all the other parts, valued at one *Louis d'or*, the pound.

N° 3. of 135 *lb.* is two feet, four inches long, three feet two inches round, very clear, excepting at the apex; valued at four florins, the pound.

N° 4. of 96 *lb.* is two feet long, two feet nine inches round, and valued at the ſame price.

And ſo on down to 10 *lb.* weight, of various qualities, proportions and prices.

A Stone *taken out of a* Horſe *at* Boſton *in* New England *in* 1724. *by Mr.* Paul Dudley. Phil. Tranſ. N° 398 p. 261.

THE proprietor of the horſe never perceiv'd, that he ail'd any thing, till within a few days before he died, and then he ſuſpected, he might be troubled with the gravel or ſtone, by the great pain the horſe ſeemed to be in, when he ſtal'd or dung'd; for, he would groan and ſweat prodigiouſly. Upon which he got a farrier, who applied ſomething to break the ſtone; but in a very ſhort time the horſe died; and the farrier being ſomewhat curious, was reſolv'd to open him; and in the great paunch he found a ſtone five pounds and a half weight, almoſt as round as a globe; for, it meaſured 17 inches round one way, and 17 inches ¾ the other. The grit was like *Newcaſtle* grindſtone, but worn ſmooth in the horſe's ſtomach, the colour ſomewhat like that of a nutmeg, but more of the ordinary millſtone. He could not procure to have the ſtone broken; but by its lightneſs, conſidering its bulk, Mr. *Dudley* was apt to think it might be porous within. How long this ſtone was generating, or what produced it, is altogether uncertain. The proprietor of it was a common carter to a griſt mill; and ſome have thought, that the horſe might either in his provender out

out of the mill, or by licking of millstones, that sometimes stand up by the side of the mill, get the first seed of this stone into his stomach. At length the weight of the stone made a fracture in the paunch, which prov'd his death: For, before the breach, and while the stone roll'd in his stomach, he was very well.

The largest stone, found in any animal that the *Philosophical Transactions* give an account of, weigh'd but four pounds, four ounces.

A Polypus *cough'd up from the* Wind-pipe; *by Dr.* Samber. Phil. Transf. N° 398. p. 262.

December 15. 1726. Dr. *Samber* was sent for to an officer of the excise in *Salisbury*, who was taken with so violent a flux of blood, that in a short time he lost near three pounds: By the time the Dr. came, it was pretty well over; only the patient seem'd to have something, when he cough'd, that stuck in the passage, which he could not get up; and by its rattling the Dr. thought it very loose. He ordered what he thought proper in such a case and left him: Next morning they told the Dr., that half an hour after he was gone, the patient had cough'd up what they shew'd him on a sheet of paper. Upon putting it into water, he found it to be a *polypus* (represented in Fig. 3. Plate VI.) and as he thinks, a very remarkable one. He could find by his blow-pipe, that it was hollow; but its being torn off with such violence, made so many holes in it, that it could not be blown up. The Dr. thinks it lin'd the *bronchia*, and that the air had a passage thro' it, and that a violent fit of coughing had separated the adhesion, and brought on that violent flux of blood, &c. The patient had been tormented with a cough for more than six months, and was gouty; but after this was cough'd up, and so large an ulcer made, he had all the successive symptoms of a fatal consumption, as cough, spitting, hectic, colliquative sweats, *diarrhœa*, and died the 16th of *January* 1727, near 50 years of age.

The Sequel of the Remarks on P. Souciet's *Dissertations against Sir* Isaac Newton's Chronology; *by Dr.* Halley. Phil. Transf. N° 399. p. 296.

DR. *Halley* in his remarks (*Phil. Tranf.* N° 397.) on P. *Souciet*'s dissertations against Sir *Isaac Newton*'s chronology, was obliged to take what he was pleas'd to give us out of *Hipparchus*'s comment on *Aratus*, not having then that author by him: Since then, the Dr. having procur'd the *Florence Edit.* of *Hipparchus Anno* 1567, finds an argument very much *ad hominem*, which P. *Souciet* must confess will bring the *Argonautic* expedition, full as low as Sir *Isaac Newton* makes it.

P. *Souciet* in his fifth *Dissert.* p. 119, 120, finds out a star of the first magnitude, close adjoining to that we now call the first star of *Aries*, as it is in the Catal. of *Ptolemy*, where it is said to be in the *horn* of *Aries*, and not in the *ear*. This star P. *Souciet* supposes to have disappear'd long since; but that being of old very considerable, it was from this first star of *Aries*, the *Zodiac* began; tho' for argument sake he is contented to let it begin as Sir *Isaac* does, with the aforesaid star in the *ear* or *horn*; which *Hipparchus* in the last and 54th page tells us, in his time followed the equinoctial colure the 20th part of an hour: And supposing the star that has disappear'd to have been at that time precisely on the colure, it must differ but 45 minutes of R. Ascens. therefrom: But how he comes to make the difference of Long. 40 minntes does no ways appear, and is *gratis dictum*.

In p. 49 of the said *Florence* edit. *Hipparchus*, treating of the rising and setting of the constellations, tells us, that that of *Aries* began to rise with 18 degrees and ½ of *Pisces* in the ecliptic, and was wholly risen with the 24th degree of *Aries*, whilst the zodiac past the meridian from 23 degrees and ½ of *Sagittarius* to 14 degrees of *Capricorn*: And again p. 52. he says, that the constellation of *Aries* began to set with the 29th degree of *Pisces*, and was entirely set with the 26th degree of *Aries*, whilst the zodiac pass'd the meridian from 29 degrees of *Gemini* to 29 degrees of *Cancer*: He also tells us, that it was the very same star that both rose and set first in that constellation, calling it p. 46. ὁ επι τȣ̃ εμπροσθίȣ ποδος; and again p. 52. it is call'd ὁ εν τοῖς εμπροσθίοις ποσιν, or that in the fore-feet of the *ram*.

The

This certainly is the ſtar P. *Souciet* would place on the equinoctial colure, and makes it long ſince to have diſappear'd; without enquiring whether the aforeſaid *data* were not abundantly ſufficient to determine its place in the zodiac at that time; and without regard to the odd uncouth poſture he muſt ſuppoſe the conſtellation of *Aries* to be in, when he makes one or both of the fore-feet ſo near to, and above the *horn* or *ear*.

Hipparchus expreſsly ſays, that it roſe when 23 degrees and ½ of *Sagittarius* was on the meridian, and ſet when 29 degrees of *Gemini* paſt it; and taking the middle between thoſe points, it is plain, that it culminated with about 26 degrees of *Piſces*, and that it had N. Declin. the exceſs above 180 degrees ſhewing, that the aſcenſional difference was about two degrees ¾. But to give the argument its full ſcope, the R. Aſcenſ. of 23 degrees and ½ of *Sagittarius* (allowing *Hipparchus*'s obliquity 23° 51′ 20″) will be found 262° 54′. And that of 29 degrees of *Gemini* will be 88° 54′: So that this ſtar was above the horizon (in the Lat. of 36 degrees N. to which *Hipparchus* has adapted his calculation) 12 hours, 24 min. or 186 degrees: Whence the R. Aſcenſ. of the ſtar is juſtly concluded 355 degrees 54′; and its aſcenſional difference preciſely three degrees; which in that Lat. makes its declination four degrees, ſeven minutes north: We have, therefore, gotten both the R. Aſcenſ. and Declin. of this firſt ſtar of *Aries*.

Let us now ſee what Long. and Lat. reſults from the aforeſaid R. Aſcenſ. with four degrees ſeven minutes N. Declin. aſſuming the obliquity with *Hipparchus*, to have been 23° 51′ 20″; and we ſhall by a juſt computation find the ſtar at that time to have been in 27° 53′ of *Piſces*, with 5° 24′ N. Lat. which, therefore, was reckoned the place of the ſtar at that time by *Hipparchus*. Add 2° 40′ for 265 years between *Hipparchus* and *Ptolemy*, and we ſhall have its place in *Ptolemy*'s account, 33′ of *Aries*, with 5° 24′ N. Lat. But the 22d ſtar of *Piſces* in *Ptolemy*'s catalogue has the ſame Long. and Lat. with ſufficient exactneſs, *viz.* 40′ of *Aries*, with 5° 20′ N. Lat. and is the middle one of the three in the north net of *Piſces*, *Bayer*'s *n*. Hence it cannot be doubted, but that this ſtar, which P. *Souciet* takes to have been once a ſtar of the firſt magnitude, was no other than the ſaid 22d of *Piſces*; which

which in *Catal. Britan.* adjusted to the year 1690, is put down in 22° 29′ and ⅓ of *Aries*, with 5° 21′ N. Lat.

How *Hipparchus* came to reckon this star to be in the fore-foot of *Aries*, does not at present appear; but it is not unlikely that these commentaries of his upon *Aratus* were written some time before he set about making his catalogue of the fix'd stars; when he might change his opinion, and replace it in the net of *Pisces*, to which it seems more properly to belong.

Be that as it will, we will for once suppose with P. *Souciet*, this star to have been in the beginning of the zodiac, or of the constellation of *Aries*, and that at the time of the first fixing the colures, that of the vernal equinox past 15 degrees in consequence thereof. Now in the beginning of 1690, this star being in 22° 29′ and ⅓, if we add thereto 15 degrees, we shall have 7° 29′ and ⅓ of *Taurus*, for the point of the ecliptic that was then the beginning of the zodiac. Now 37 degrees and ½ at 50 seconds *per annum*, gives 2700 years; from which deducting 1690, we shall have 1010 years before Christ. But this star having 5° 21′ N. Lat. the colure when it past over it, intersected the ecliptic in 2° 20′ less longitude, which gives the time 168 years later, or but 842 years before Christ. So that *notwithstanding this grand discovery*, the new system of chronology is so far from being refuted, that it seems to be very much confirm'd thereby, at least in the opinion of P. *Souciet*.

Dr. *Halley* has assum'd the latitude to which *Hipparchus* might have adapted his calculations to be 36 degrees; because he finds in p. 14. of the aforesaid edition, that he makes the longest tropical day 14ʰ 30′. And in p. 29. he tells us, that the southern star in the left foot of *Bootes*, *Bayer*'s υ, having 27° 20′ N. Declin. was above the horizon 14ʰ 57′: Whence it follows, that the Lat. must be 36° 5′. He also tells in the same p. 29. that this star set when 22° of *Capricorn* culminated and 6° of *Taurus* ascended; repeating the same thing in p. 39. which leaves no room to suspect, that those numbers are not the same that *Hipparchus* had computed; the Dr. therefore, thought it worth while, to enquire in what latitude six degrees of *Taurus* rises, when 22 degrees of *Capricorn* is on the meridian; and with the obliquity of the ecliptic, as now we have it, the latitude resulting is 35° and ½ N. but with the obliquity allow'd by *Hipparchus*, it will be found less than 35°.

This

This the Dr. says only to obviate any objection that may be made by P. *Souciet* to the foregoing argument; tho' if he please to examine it, he will find that an error of a degree in the assum'd Lat. will by no means invalidate the proof here given, that this first star of *Aries*, could be no other than the middle one in the northern net of *Pisces*, mark'd *η* by *Bayer*.

An Account of the Lumen Boreale *observ'd at several Times; by Dr.* Langwith. Phil. Trans. N° 399. p. 301.

JAN. 4. 1726-7. There was observ'd a luminous arch, which extended itself from N. E. to W. The streams mov'd all westward. The wind N. W. The mercury 29 and ½.

Jan. 5. There was something of the same nature, but hardly enough for observation; and yet this very night the appearances were more remarkable in some parts of the kingdom than those of *Oct.* 8. 1726. This Dr. *Langwith* was inform'd of by a person of quality in *Lancashire*, who sent him the annex'd description and draught, communicated to him by a curious observer at *Liverpool*.

March 2. Between 7 and 8 there was an arch, upon a black basis as before, extending itself from N. E. to W. Its height variable; pyramidal streams of greenish light moving westward: About a quarter past eight, there shot up from the west a stream of pale flame colour, about six or seven degrees broad: It pass'd over the *Pleiades*; and crossing the meridian about 19 degrees to the north of our zenith, descended as low as the tail of *Ursa Major*, which it left a little to the south. It continued thus for some minutes, and then gradually vanish'd: The wind north: The mercury about 30.

March 3. The appearances this night were extraordinary; the Dr. chiefly takes notice of such particulars, as differ'd from those of *Oct.* 8. 1726.

1. That instead of one luminous arch in the north, here were two, and sometimes three one above another; they were distinct enough from each other in their upper parts, but blended together towards the horizon, which they generally intersected about N. E. and N. W. but sometimes varied considerably from those points.

The same observation may be applied to the heights; for, they were also variable; and in particular, the inner edge of the

the lowermoſt arch was at ſometimes about ſix degrees above the horizon, at others, conſiderably more or leſs.

The Dr. ſuppoſes this extraordinary appearance was owing to ſeveral diſtinct collections of luminous vapours, which were either at different heights from the earth, or different diſtances from the eye.

2. Several of the more permanent ſtreams were bent at times into irregular arches of different curvatures and poſitions.

Some of them held pretty near the ſame ſhape till they vaniſh'd; others went off moſt commonly in tangents to ſome part of the former curves.

3. The flaſhing ſtreams from the eaſt ſometimes met with thoſe from the weſt; and ſo form'd continual arches of a pale colour, which quickly broke and vaniſh'd: No colouring followed upon the mixture of theſe ſtreams.

4. The ſtreams of this kind mov'd moſtly ſouthward, but not to any certain point; for, they were inclin'd to the horizon at all degrees, between 5 or leſs and 90. There was ſometimes ſuch an odd irregularity in their motions, as can hardly be deſcrib'd; for, the places whence the flaſhings were directed ſeem'd to vary every moment.

As to the more ſteady pyramidal ſtreams, they generally mov'd weſtward; and tho' ſome of them at times ſeemed to ſtand ſtill, or even move backward, yet the Dr. is apt to think this irregularity was only apparent.

5. A little after the beginning of this meteor there was a faint ruddineſs in the ſky towards N. E. and N. W. but when it was in its greateſt perfection, towards 12 o'clock, the Dr. obſerv'd none of the priſmatic colours, tho' the air was then full as light as he ever knew it upon thoſe occaſions. This helps to confirm him in his opinion, that the priſmatic colours in theſe meteors were owing to the ſun.

Theſe appearances began early in the morning, and held, as the Dr. was inform'd, for a good part of the night. The wind was north weſterly: The mercury above 30.

The Dr. was inform'd that theſe meteors are much more common in the north than in any other parts of *England*, and that they are call'd *ſtreamers*, *merry dancers*, or *petty dancers*: They alſo pretend to foretel the weather by them, and ſay, that when the ſtreamers are green, they betoken wet ſtormy weather; but when they are yellow, it will be clear and dry.

Ariſtotle

Aristotle has given an imperfect account of some of these meteors.

The description of the *aurora borealis*, observed at *Liverpool*, is as follows.

Jan. 5. 1726-7 about 7 o'clock at night, the author was told that the meteor called by our sailors, *merry dancers*, was visible and very bright. Having observed several before, but having no opportunity of being particular in his observation, he went out into the open air, clear of houses, that he might have a better view all round the horizon, from whose northern part arose several streams of light, from behind a black cloud, as it were. They were very numerous, and he believes there was no possibility of reckoning them, their motion being so quick, shooting upwards to the zenith, with a motion not to be followed by the eye. They had also another motion which seemed to be side-wise; their higher ends terminating sometimes in a sharp point, sometimes in two or three points; they appeared from the north-west to the north east; but were brightest in the north: Their colour was pale like that of *Jupiter* thro' a telescope, but not near so bright. Most of them reached the zenith, where mixing with each other, they whisk'd round, and formed an appearance like the curling flame of a glass-house fire; they had a very irregular motion, sometimes turning inwards, sometimes outwards, like the spring of a watch. This circular light was the brightest, and seemed to occupy near 10 degrees of the highest part of the hemisphere; several strokes of light seemed to dart from it to the south; but died away before they got any considerable distance: In the west he observed two small long clouds, which interposed between him and the light streams, which he observed above the clouds and between them; which convinced him, that this light (whatever it be) is far above them.

Fig. 4. Plate VI. represents the whole horizon, as it appeared to him: The bright star is *Jupiter*, whose place then was in 17° of *Aries*, and was about south-west, he conjectures about 20 degrees high. Some of the brightest stars in *Taurus*, *Orion* and *Aries* appeared south and south-east; but he has placed those only by conjecture: In this state he left it; but he was informed by one that saw it after 10 o'clock, that the whirling light in the zenith appeared of several colours, as blue, green, yellow, reddish.

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A shock of an Earthquake *felt near* Dartford *in* Kent; *by* *Mr.* Edmund Barrel. Phil. Transf. N° 399. p. 305.

MR. *Barrel* was informed that the earthquake was felt very sensibly at a farm on a hill, called *Skeat-hill*, which is at the west-end of *Lullingstone-park*, belonging to Mr. *Percival Hart*, about eight miles south west from *Dartford*; and that same morning a piece of ground in a meadow in *Farningham*, about five miles south of *Dartford*, fell in; so as to leave a pit about 8 or 10 feet over, and nearly of the same depth; and being on the same level with the river, it was (when seen that morning) filled with water, within 3 or 4 feet of the top; tho' that spot of ground was supposed to have been as sound, as any about it, carts having several times gone over that very place.

A subterraneous Fire, *observed in* Kent; *by Dr.* Nesbit. Phil. Transf. N° 399. p. 307.

THIS subterraneous fire was first taken notice of on *Aug.* 2. 1726. in a marshy field, situated in the parish of *Flinx-hill*, about 10 or 11 miles south-west of *Canterbury*.

It began on the side of a little brook near the water, and continued to burn along its bank, without spreading much for some days: Afterwards it appeared on the other side, and extended itself the space of some acres over the field, consuming all the earth, where it burnt, into red ashes quite down to the springs, which in most places lay four feet deep and upwards. On the 24. of *Sept.* Dr. *Nesbit* went to see it, and found it had consumed, as well as he could conjecture by his eye, about three acres of ground.

It then burnt in several places, and emitted a great smoke and strong stench, very like that of a brick-kiln; it never flamed, but when the earth was turned and stirred: For some space round where it was burning, the ground felt hot, tho' the grass seemed no more parched than might reasonably be expected from the dryness and heat of the season. He caused it to be turned up in several places, and found the earth hot and wet near four feet deep, and much hotter about two feet than near the surface.

When the earth was exposed to the air, tho' it was very moist, and not hotter than one might easily bear with his

hand, the heat increased so fast, that in a few minutes it was all over on fire, like *phosphorus*, made with alum and flower.

The soil of the field is of the same nature with that they make the turf of in *Holland*; the surface of it is always wet, except in exceeding dry seasons: This year it was somewhat more parched, and harder than usual.

From what has been related the Dr. thinks, it is not more difficult to account for this fire, than for those which often happen in hay-ricks; when hay is stacked before it is thoroughly made.

Experiments on the Effects of the Poison *of the* Rattle-snake; *by Capt.* Hall. Phil. Trans. N° 399. p. 309.

IN *South Carolina May* 10. 1720, Capt. *Hall*, having procured a fine healthful rattle-snake about four feet long, did with three or four Gentlemen and one Mr. *Kidwell* a surgeon, make some experiments on the effects of its poison.

They procured three curr dogs, the largest not bigger than a common harrier; and the least about the bigness of the largest siz'd lap-dog, all of them smooth hair'd.

The snake being tied and pinned down to a grass-plat, they took the largest of the dogs, which was a white one, and having tied a cord round his neck; so as not to strangle him, another person held one end, while the Captain held the other; the length was not more than four yards each way from the dog.

Immediately on their bringing the dog over the snake, he raised himself near two feet, and bit the dog, as he was jumping; the dog yelp'd, by which the Captain perceived he was bitten; upon which he pull'd the dog to him, as fast as he could, and perceived his eyes fixt, his tongue between his teeth, which were close, his lips drawn up in such a manner, as to leave his teeth and gums bare: In short, the dog was quite dead in $\frac{1}{4}$ of a minute, as the by-standers judged; but one person, besides the Captain, was of opinion it was in $\frac{1}{2}$ that time. They could not see where the dog was bitten, nor observe any blood; upon which ordering some hot water to scald the hair off, they could discover but one puncture, which looked of a blueish green colour a little round it; it was just between his fore-leg and his breast, where, when his legs are extended, the hair is much thinner than in some other places.

Half

Half an hour after the firſt bite they took a ſecond dog, which was ſomewhat leſs, of a liver-colour, and in like manner brought him over the ſnake, which in a very little time bit his ear, ſo that they all ſaw it; the dog yelp'd very much, and ſoon ſhewed the ſigns of being very ſick, holding the ear that was bit uppermoſt: He reel'd and ſtaggered about for ſome time; then he fell down, and ſtruggled, as if convulſ'd, and for two or three times got up, each time wagging his tail, tho' ſlowly, and attempting to follow a negro-boy, who uſed to make much of him. They put him into a cloſet, and ordered the boy to look after him.

About an hour after the ſecond was bitten, they took the third dog in like manner: The ſnake bit him on the right ſide of the belly, about two inches behind the long ribs; for, they obſerved he had drawn blood there: The dog for about a minute did not ſeem to be hurt; ſo they let him go, ſince they could get him again. For that day they put up the ſnake, imagining his poiſon was very near, if not quite, expended.

Two hours after the ſecond dog was bit, the *Negro* boy told them he was dead.

About an hour after Mr. *Kidwell* opened him, in order to examine the heart; but the Captain could not perceive any remarkable difference between that and ſeveral others he had ſeen, where there was no poiſon in the caſe. Mr. *Kidwell* laid open the ſcull, and was of opinion, that the brain was more red and ſwoln than any he had ever obſerved; and he told the Captain a little while after, that the blood turned very black.

For that day they heard no more of the third dog which was bitten; but next morning the woman who own'd him came to the Captain, complaining of his cruelty for killing her dog: She did not know when he died, but ſaid ſhe ſaw him at 7 in the evening before, which was about 3 hours after he was bit; and that he was ſo ſick he could ſcarce wag his tail. None of thoſe dogs were ſwoln before they died.

On the 14. they got two dogs, both as big as common bull dogs. The firſt dog, which he bit on the inſide of his left thigh, died in $\frac{1}{2}$ a minute exactly, in the opinion of two Gentlemen, who kept their watches in their hands all the while: There were two very ſmall punctures in his thigh, which looked livid, tho' no blood was drawn. This dog did not ſwell for four hours after he was dead.

 The

The ſecond dog was bit about an hour after the firſt, on the outſide of his thigh, where they perceived the blood at two places: He ſoon ſickened, and died in four minutes.

They thought the ſnake's poiſon was not ſpent; ſo got a cat (for, they could procure no more dogs) which he bit about an hour after, tho' the Captain could not ſay where: The cat was very ſick, and they put her up in a cloſet: By ſome means the cat was let out in leſs than an hour and a half after ſhe was bitten. Next morning early ſhe was found dead in the garden, and much ſwoln; ſo that no body car'd to examine, or ſearch where ſhe was bit.

About ¾ of an hour after he had bitten the cat, he bit a hen twice; the hen ſeemed very ſick and drooping, and could not, or did not fly up to her uſual place of rooſt among the reſt that night; but the next day ſhe ſeemed very well, and continued ſo till evening, when the Captain ordered her to be killed, and her feathers ſcalded off: There were two punctures in her thigh, and a ſcratch on her breaſt over the craw; all which looked livid.

About a week after, having got a large bull-frog, they brought him over the ſnake, as uſual, which he bit with a great deal of force; ſo that he ſeemed to faſten for a ſmall time: The frog died in two minutes or thereabouts.

In leſs than a quarter of an hour after he bit a chicken, which was hatched the *February* before, that died in three minutes; the Captain could not ſay where it was bit, and he was at a loſs to try any farther experiments for a long time, for want of proper ſubjects.

About the middle of *June* the Captain took the ſnake out according to cuſtom; and having procured a common black ſnake, not of the viper kind, about two foot and a half or near three foot long, that was healthful and juſt taken; he put them both together, and irritated them, ſo that they bit each other; and he perceived, that the black ſnake had drawn blood of the rattle-ſnake, before he took them aſunder.

In leſs than eight minutes the black ſnake was dead; and he could not perceive the rattle-ſnake at all the worſe for it, or ſick.

On the laſt day of *June* he took the rattle ſnake out to try, whether if he bit himſelf, it would not prove mortal to him. The Captain hanged him in ſuch a manner, that he was not above half his length on the ground; and with two needles at the

the end of a stick, one to prick, and the other to scratch, irritated him so much, that he soon bit himself, after having several times attempted to bite the stick: He then let him down, and he was quite dead in eight minutes, or thereabouts; but he is sure it did not exceed 12 minutes.

A Gentleman, upon cutting the snake into five pieces, gave them to a hog, the head part first, in sight of several people: The hog eat up all the snake; and 10 or 12 days afterwards the Captain saw the hog alive and healthful.

This was no more than what the Captain had observed before; but doubted they had taken some other snake for a rattle-snake: For, being at the house of Mr. *Charles Hart*, he was shewn a snake, which a *Negro* told him he had killed just before; it was in three pieces, the head of it bruised into the ground: While the Captain was looking on, a sow that had pigs following her, came and eat it up very greedily.

The Captain never heard she was sick for it, tho' he enquired; and about 10 days after he saw her in very good health. He has heard 50 relations of the same kind; and was informed, that those hogs, which feed in the marshes, will run after the common sort of water snakes, which are not poisonous, and will feed on them greedily: And in *Maryland* in *Aug.* 1717, he saw a hog eat up the head of a rattle-snake just cut off, and while it was gasping very dreadfully; and he was told it was a common thing, and it would do them no harm.

June 10. 1723, Mr. *Thomas Cooper*, who practises physic at *Charles-town* sent the Captain word, that he had procured a fine rattle snake, which had been taken not above four days, about three foot and a half long, and that he designed to try, whether he could save some of the dogs after the snake should bite them. He provided a large quantity of *Venice-treacle*, or mithridate (the Captain could not be positive which) and divided it into two potions, each about two ounces; to one of them he put a large quantity of diaphoretic antimony.

The first dog which the snake bit on the inside of the thigh, died so soon, *viz.* in about half a minute, that they could not get the potion (which was that without the antimony) down his throat soon enough to expect it could have effect.

Upwards of an hour after, the second dog was bitten by the snake, and had two punctures, or holes in the fleshy part of the inside of his left fore-leg; which bled more than any the Captain had observed before: They immediately got down his throat that preparation with antimony; he soon grew very sick, and strove

ſtrove to vomit; but brought up very little, if any at all, he frothed at the mouth, and bit at the graſs; which he champ'd, as if he were mad: and indeed they were all afraid of him: They, therefore, put him into a room, and kept him there till next morning, where the Captain ſaw him, as he thought, recovered. They throw'd him ſome meat, which he eat; ſo letting him out, he went home. About a month after that, the dog's pile came off, and his maſter kill'd him, being ſo ugly to look at; for, he told the Captain he looked like (as he call'd it) a leprous perſon. The Captain never heard that this dog ſwell'd.

The third dog the ſnake bit was a ſhaggy ſpaniel, about an hour and a quarter after the ſecond. He was bitten on the foremoſt part of his right ſhoulder, as they perceived by the blood. The dog ſeemed to bite at the place himſelf, and was very ſick for about two or three hours; but without any means or application, he recovered, and the Captain never heard he was ſick afterwards.

Obſervations on the Brains *of three* Perſons, *who died of the* Epilepſy; *by Dr.* Rhætus. Phil. Tranſ. N° 399. p. 315. *Tranſlated from the* Latin.

A Man 35 years of age, of a thin habit of body, being ſeiſed two years before with that ſort of pain, called *gravativus*, in the anterior part of the head towards the forehead, took ſeveral medicines, but to no purpoſe; ſome time after, he had a plentiful hemorrhage at the noſe, which ceaſed ſpontaneouſly. After the hemorrhage, he loſt the ſenſe of ſmelling, and afterwards he was ſeis'd with frequent returns of epileptic paroxyſms for two years together, and at length he died.

Upon opening the ſcull, Dr. *Rhætus* obſerved the anterior part of the brain on the right ſide towards the *dura mater*, and about the region of the *criſta galli*, hard and callous, and very cloſely adhering to the *dura mater*; in the anterior part of the brain towards the left hand, there was a ſmall quantity of extravaſated bloody matter: All the other parts were in a natural ſtate.

A woman 60 years of age, labouring under an epilepſy, and being ſeiſed with very violent and frequent fits, died thereof.

Upon opening the ſcull, the Dr. found between the *pia mater*, and brain, as alſo in the anterior ventricles of the brain, a pretty large quantity of extravaſated *lympha*; ſo that the ventricles

ventricles were almost turgid therewith: In the *plexus choroides* there were a great many whitish, pellucid, bodies of various sizes, containing a waterish *serum*; the largest were near as big as large hemp-seed.

Upon opening the scull of an old woman, that for several years together had been very much subject to the epilepsy, the Dr. observed between the *pia mater* and brain a small quantity of extravasated *serum*; and the anterior ventricles of the brain were so distended therewith, that they were ready to burst; both the *plexus choroides* appeared like a cluster of grapes, and in them there presented to view a great many round, pellucid vesicles, of various sizes, that resembled very elegant pearls.

The Substance of a Cataract; *by the same*. Phil. Trans. N° 399. p. 317. *Translated from the* Latin.

AN old woman about 50 years of age had a true cataract in the left eye, of a pearl-colour, and of an ordinary size; yet so large as to possess a little more than the half, and that the middle part of the pupil: He plainly observed it behind the *uvea*, and suspected it was in the second cell of the eye. And the Dr. being asked by *Santorini*, in the presence of about 10 physicians, what he thought of this cataract; whether he took it for a pellicle or some defect in the crystalline humour? He made answer, that he rather took it for the former; yet that he would not absolutely and certainly affirm it, because there is no pathognomonic sign: Upon which *Santorini* smil'd, that he should venture to determine any thing about a pellicle or membrane; for, *Santorini* was persuaded of the truth of *Heister's* system.

Upon dissecting the eye, the crystalline humour was found to be pellucid; its whole substance, it is true, was slightly and equally tinged with a citron colour, of which there appeared scarce any sign, whilst the woman was alive; the pellicle was loose in the said cell, only that it adhered to the *uvea* by two very fine fibrils: The pupil was likewise found to be somewhat larger than in a natural state.

Two chirurgical Questions *stated, and answered; by Mr.* John Douglass. Phil. Trans. N° 399. p. 318.

Q. 1. WHether it be not possible in some measure to relieve those persons (who by reason of their great age, bad habit of body, &c. cannot submit to any of the great operations

operations for the stone with tolerable hopes of success) by making an artificial *fistula* in the *perinæum*?

A. Daily experience shews, that a great many patients, both young and old, offer, who are afflicted with the stone in the bladder, whom we cannot with any tolerable hopes of success, advise to submit to the great operations for the stone. Is there no medium hitherto found out between living in extreme misery, and submitting to a desperate operation? Yes, *Thomas Fienus*, about 125 years ago, proposed a palliative cure for such patients, where a radicative cure could not be expected; an operation which may be performed with safety on the oldest; the wound is so small, and the parts cut of so little consequence to life; an operation by which we can prevent or alleviate the lamentable effects of the stone, *viz.* The total suppression of urine, and the cutting pains they endure both in, and after making water, *&c.* Yet this operation has been as little minded all this while by the Hospital Lithotomists, as *Rosset*'s most excellent treatise, before Mr. *Douglas* introduced the hypogastric section in 1719. Tho' he admires *Fienus*'s design in making this *fistula*; yet he can by no means approve of his way of doing it.

It should be performed thus; place the patient, as in *Marianus*'s operation; pass a staff into the bladder, then cut the skin and fat, till you lay that part of the *urethra* bare, which reaches from the prostate *gland* to the cavernous *urethra*, then make a small incision into it with the point of your knife; then withdraw the staff, and pass a small flexible *canula* into the wound of the *urethra*; then dress the wound. S. A. extract the *canula*, clean it, and introduce it again every dressing; so as to leave a *fistula* in the room of the wound.

Thro' this *fistula* the patient himself, or any one about him, may easily pass an oiled probe, and push the stone back, whenever he finds himself attacked with a suppression of urine, or when the stone presses hard against the *sphincter*, when endeavouring to make water, which otherwise could not be done, without sending for, or staying in misery, till a surgeon came to pass the *catheter*, which in such cases is not always to be done, without a great deal of pain, and sometimes danger. By this *fistula* we can likewise very easily inject any liquor that may be thought proper, either to prevent, or allay the inflammation of the bladder, or cleanse it from the gravel, or any other sort of filth, that may collect there, by which the increase of the stone will be prevented, *&c.*

In

In females all thoſe advantages are obtained by the natural ſtraitneſs and ſhortneſs of the *urethra*; whence they never ſuffer the tenth part that males do; which is an inconteſtable evidence, that when the paſſage into the bladders of males is made as ſtraight, and near as ſhort (as is done by the abovementioned *fiſtula*) they will reap the ſame advantages by it.

Artificial *fiſtula's*, therefore, ſhould be made for ſuch, as by reaſon of their great age, and bad habit of body, &c. cannot undergo any of the great operations for the ſtone, with any hopes of ſucceſs.

Q. 2. Whether it be not poſſible to dilate the artificial *fiſtula* in the *perinæum* of males, and the *urethra* of females, with ſpunge or gentian-tents, gradually increaſed for ſome time, to ſuch a width, that we may eaſily paſs a pair of *forceps* into the bladder, with which, the ſtone, when ſmall, may be extracted; and when large, or of an irregular figure, broken, and the pieces extracted gradually, and at different times, when they cannot be extracted at once, without fatiguing the patient too much?

A. To prove that both theſe *fiſtula*'s may be dilated to a ſufficient bigneſs by the means propoſed (eſpecially, if the parts be frequently bathed in a *ſemi-cupium*, or otherwiſe, as the operator ſhall think proper, and ſome warm oil injected into the *fiſtula* every time the tent is changed (the better to ſupple, and relax the parts) Mr. *Douglas* makes uſe of three arguments only, *viz.* one from common experience in analogous caſes; another from the operations of nature on the ſame parts; and a third from inſtances of this operation being performed after the method he propoſed.

1. Common experience ſhews to what a great width *fiſtula's* in all other parts of the body, tho' very ſmall at firſt, may be dilated by ſpunge or gentian-tents.

2. Nature herſelf, without any art, has frequently performed this operation on both ſexes; in males who have been cut for the ſtone the old way, and have had *fiſtula*'s remaining in *perinæo*. It is frequently found, that ſome conſiderable time afterwards, ſtones of no ſmall ſize have appeared, which made their way thro' the *ſphincter* of the bladder into the membranous *urethra*, and ſtuck near the orifice of the *fiſtula*, whence they were eaſily and ſafely extracted. Mr. *Douglas* ſaw a ſtone as big as a pullet's egg, that was expell'd from the bladder of a young woman without any help, as her phyſician aſſured him, and ſhe had no inconveniency afterwards, which certainly

would have happened, if it had been extracted after the common violent method. Dr. *Beard* gives an account of a still larger stone, that passed after the same manner, but the patient had the common inconveniency afterwards, *viz.* an incontinency of urine, which was owing more to the roughness than to the largeness of the stone, which had lacerated the parts, as in the common operation; which might have been prevented, had she been assisted in time, as above.

3. Mr. *Collet*, in his *Traité de la taille*, gives an account of a Gentleman, on whom he performed this operation three different times, and extracted in all ten stones; his words are to the following purpose.

'In fine, M. *Usson* being out of danger, and his wound near 'cicatrising; I advised him, very well foreseeing what would be 'the consequence, to keep the wound open, and only have a 'small *canula* therein, which on occasions would give full li'berty of making injections in order to clean the bladder from 'all filth, and to extract any new stones that might be formed. 'The patient did accordingly, and he found himself well for 'five years, he afterwards lived in pretty good health. But at 'three different fits, I was obliged to extract about ten stones 'that had formed themselves in the bladder: A small spunge 'tent being put for some hours in the *fistula*, instead of the *ca'nula*, facilitated my introducing a very small pair of *forceps*. 'M. *Usson* immediately put on his cloaths and went abroad 'about his ordinary business.'

Mr. *Douglas* knows a Gentleman that keeps a *fistula in perinæo* open for the very same reason.

Since then it is evident, that *fistula*'s in all parts of the body are dilatable to a considerable width; since nature can often of herself dilate the very parts in dispute to a very extraordinary degree; and since this very operation Mr. *Douglas* here proposes has been successfully performed three times on the same person.

Artificial *fistula*'s in males, and the *urethra* in females may, therefore, be dilated in such a manner, as to extract any stone, without cutting the body of the bladder, or lacerating any of the parts.

N. B. This operation will appear less surprising, if we consider, that it is only dilating the *sphincter* of the bladder, *&c.* in a different manner from that daily done in *Marianus*'s operation, *i. e.* by this method the parts are dilated gently and gradually, after being well bathed and anointed, the better to relax them: Whereas in *Marianus*'s operation they are dilated

quickly

quickly and violently; and then the ſtone, let it be of what largeneſs or figure ſoever, is forcibly and immediately extracted: Whence ariſe contuſions, lacerations, hemorrhages, inflammations, mortifications, &c.

A Solar Eclipſe *at* Lisbon Sep. 25. 1726. *N. S. by F.* Carbone. Phil. Tranſ. N° 400. p. 335. *Tranſlated from the* Latin.

THE ſky being, contrary to expectation, very clear, F. *Carbone* obſerv'd this eclipſe at *Lisbon*, but not the end; for, a hill interpoſing, the ſun was hid about a whole minute before it fully appear'd again. He made uſe of a teleſcope eight *Paris* feet in length, and fitted with a very accurate micrometer. Yet he met with ſome inconveniencies in his obſervations, whereby he could not inveſtigate the phaſes of ſome few digits, or duly mark them. Notwithſtanding, he thinks, that what obſervations, he had an opportunity of making, are without any ſenſible error; both becauſe he omitted no care in daily meaſuring the ſolar digits, and becauſe he very accuràtely diſcover'd the correct time by repeated obſervations the ſame day.

To confirm the ſaid correct time, he ſubjoins two altitudes of the ſun, one of which he took before the beginning of the eclipſe, and the other about the end, with an aſtronomical quadrant of three *Paris* feet radius, and both entirely agree with the obſervation at noon, and argue the ſame error of the clock.

	h.	'	"
At noon the clock ſhewed	11	58	34
Differing from the true time		1	26

	o.	'	"
The true altitude of the ſun's centre after noon	28	53	45
His ſouthern declination		53	18
The altitude of the pole aſcertain'd by ſeveral obſervations, at leaſt as to minutes	38	42	0
From which *data* the arch of the ſun's diſtance from the meridian is found by trigonometry	59	49	36

 Which

	h.	′	″
Which, if converted into time, gives	3	23	18
And the clock ſhew'd	3	21	48
It, therefore, differed from the true time		1	30

	°	′	″
Again the true altitude of the ſun's center	4	52	54
His ſouth declination		55	24
The altitude of the pole, as above	38	42	0
From which again the arch of the ſun's diſtance from the meridian is inferr'd by trigonometry	82	59	40
	h.	′	″
Which converted into time gives	5	31	59
And the clock ſhew'd	5	30	26
Conſequently, it differed from the true time		1	33

He, therefore added this equation to the time of the clock, in order to make the true or apparent time correct, and this he made uſe of in the following obſervations.

Phaſes of the immerſions.	true time		
	h.	′	″
The limb of the moon begins to touch that of the ſun, doubtful	5	59	50
One digit eclipſed — —	4	5	10
Two digits — —	4	10	45
Three digits — —	4	16	55
Half the fourth digit — —	4	20	14
Four digits — —	4	23	48
Five digits — —	4	31	5
Half the ſixth digit — —	4	34	51
Six digits — —	4	38	57
Seven digits — —	4	47	57
Half the eighth digit — —	4	54	59
The time of greateſt obſcuration as far as could be gather'd from the other phaſes was	4	58	30

The

The quantity of greatest obscuration was 7 dig. 45'.

Phases of the emersions.	true time		
	h	'	"
Half the seventh digit eclipsed	5	13	40
Six digits	5	17	58
Five digits	5	25	58
Half the fifth	5	29	43
Half the fourth	5	36	49
Three digits	5	40	8
Half the third	5	43	14
Two digits	5	46	10
Half the second	5	48	58
One digit	5	51	40
The inferior limb of the sun touches the hill	5	52	42
The sun entirely hid, being still eclipsed about 12 minutes, namely $\frac{1}{5}$ of a digit	5	55	27
The apparent altitude of the hill, or of the superior limb of the sun 57' 30", which corrected, will be 34' 29"; from which substracting the sun's semi-diameter 16' 4", there will remain the true altitude of the sun's center, when entirely hid, *viz.* 18' 25": Whence is inferr'd the time corresponding to the said altitude, namely.	5	55	27
The true setting of the sun	5	57	1
His apparent setting, on account of the refraction, about	6	0	0
Whence it appears, that the end of the eclipse was visible in their hemisphere; for, it happened, as far as could be gathered from the above observations, about	5	56	50

A Lunar

A Lunar Eclipse *at* Lisbon, Oct. 10. 1726. *N. S. by the Same.* Phil. Transf. N° 400. p. 338. *Translated from the* Latin.

ABOUT midnight the clouds seemed to disperse, and at two o'clock in the morning the western parts of the heavens, to which the moon tended, appeared very serene, and continued so till the end of the eclipse; F. *Carbone* had therefore, this observation without any inconveniency from the sky; nor did he omit any thing, which he thought would any wise favour its exactness. He made use of a telescope eight *Paris* feet in length, and fitted with a very accurate micrometer.

To measure the time he made use of a pendulum clock, the equality of whose motion was such, that in eight or ten days he found it differed scarce one or two seconds of time from the mean motion of the sun.

The names of the *maculæ* are taken from P. *Grimaldi*'s *Selenographia*, which P. *Riccioli* has inserted in his *Almagestum novum*, and enrich'd with names. But for the benefit of such as follow *Hevelius*'s way of denominating them, F. *Carbone* likewise subjoins the synonymous names from this author, perfixing the letter H, which denotes *Hevelius*.

True time correct. h.	′	″	Phases.
14	37	0	The sensible *penumbra* begins to tinge the moon's limb to the south-east.
14	46	0	It appears more dense.
14	56	0	Very dense.
14	57	20	The earth's shadow, as far as could be discern'd, touches the said limb.
15	3	50	*Schickardus* immerged.
15	4	53	I digit eclipsed.
15	8	4	The shadow at *Kristmannus*.
15	9	15	Entirely immerged.
15	9	50	*Mersennus* covered: The eastern shore of *Mare humorum* begins to be obscured: H. *Sinus Sirbonis*.
15	13	0	II digit eclipsed.
15	13	36	*Capuanus* immerged.

The

True time correct. h.	′	″	Phases.
15	14	54	The ſhadow at *Grimaldus*: H. *Palus Maræotis.*
15	17	34	*Gaſſendus* begins to be obſcured.
15	19	46	*Tycho* begins: H. *Mons Sinai.* *Grimaldus* covered.
15	21	0	Half of *Tycho* immerged.
15	21	53	III. digits. *Tycho* entirely immerged.
15	23	58	*Morinus*: H. *Caſſius.*
15	25	20	*Bullialdus*: H. *Inſula Creta.*
15	30	58	*Prophatius* begins to be obſcured.
15	31	48	IV digits eclipſed.
15	32	40	*Prophatius* covered.
15	43	40	V digits eclipſed.
15	48	0	The ſhadow almoſt touches *Galilæus*, near which it continues for ſome time.
15	53	20	The ſhadow at *Snellius* and *Furnerius.*
15	58	30	*Fracaſtorius* entirely immerged.
16	3	4	VI digits eclipſed.
16	6	13	The ſhadow touches *Sinus æſtuum*: H. *Mare Adriaticum.*
16	8	22	The middle of *Vendelinus.*
16	14	40	*Mare Nectaris* entirely immerged: H. *Sinus extremus.*
16	15	51	*Grimaldus* begins to emerge.
16	16	18	*Ricciolus* emerged.
16	23	30	*Grimaldus* entirely emerged.
16	24	27	The ſhadow at *Langrenus.*
16	43	34	*Gaſſendus* emerged.
16	47	22	V digits eclipſed.
16	51	57	*Bullialdus* begins to emerge.
16	53	47	Entirely emerged.
16	54	17	*Kriſtmannus* begins to emerge.
16	58	0	As alſo *Schickardus.*
16	59	27	IV digits eclipſed. *Schickardus* entirely emerged.
17	3	30	*Pitatus* entirely emerged.
17	9	20	III digits eclipſed.
17	10	54	*Tycho* begins to emerge.
17	11	49	The middle of *Tycho* emerged.
17	12	40	*Tycho* entirely emerged.

Fraca-

True time correct.			Phases.
h.	′	″	
17	14	20	*Fracastorius* entirely emerged.
17	18	4	II digits eclipsed.
17	5	43	*Snellius* emerges.
17	26	10	I digit only eclipsed.
17	27	55	*Furnerus* emerged.
17	32	50	The extreme limb of the moon, regarding the south, seems next to emerge.
17	33	30	Now the entire disk of the moon appears; and consequently, the end of the eclipse.
17	38	0	The said limb of the moon is tinged with a still denser *penumbra*.
17	54	0	The sensible *penumbra* ends, and the moon appears entirely restor'd to her former lustre.

h.	′	″	
16	15	25	The mean or greatest obscurity.
			From the observations of the beginning and end of the eclipse its duration is gathered.
2	36	10	The quantity carefully found by the micrometer was 6 digits 10 min.

Remarks on some Experiments in Hydraulics, *which seem to prove that the Forces of equal moving Bodies are as the Squares of their Velocities; by Mr.* Eames. Phil. Transf. N 400. p. 343.

THE result of these experiments is, that the velocities of any fluid, issuing out at equal orifices, made in the sides of vessels, fill'd up to different heights, and kept full at those heights, above the orifices, are found to be as the square roots of those heights respectively. Thus, when the different heights above the orifices are as the numbers 1, 4, 9, 16, &c. the velocities of the particles of water, issuing out, are found to be as the numbers 1, 2, 3, 4, &c.

The argument drawn from these experiments, in favour of the opinion, that the forces of equal masses, or moving bodies, are proportional to the squares of their velocities, runs thus: All the particles of water being of the same nature,

nature, and uniform, every ſingle particle iſſuing out with two degrees of velocity, muſt move with four times the force of any other ſingle particle, that moves but with one degree of velocity; becauſe the force with which it moves, is the effect of a cauſe four times greater; namely, the preſſure of a column of water, whoſe height is four times greater.

Thus again, a particle of water running out with three degrees of velocity, muſt move with nine times the force of a particle moving but with one degree of velocity; becauſe that force is the effect of a cauſe nine times greater, *viz.* the preſſure of a column nine times higher: Since no leſs than a column nine times higher is found by experience neceſſary to make the ſeveral particles of water iſſue out with three degrees of velocity. So that in theſe two inſtances, it ſeems to be certain, that the forces communicated are, as the ſquares of the velocities: And that it is ſo univerſally, is argu'd thus: The preſſures are as the altitudes, and the altitudes as the ſquares of the velocities of every ſingle particle; therefore, the preſſures are as the ſquares of the velocities: But the preſſures are the cauſes of the forces, with which the ſeveral particles of water iſſue out, or move; and therefore, ſince effects are proportional to their cauſes, the forces with which the ſeveral particles iſſue out, and move, are as the ſquares of the velocities.

Remark I. The fault in this reaſoning, and which quite runs thro' it, is the miſtaking a part of the effect for the whole. The entire effect of any of theſe preſſures is not barely a certain number of degrees of velocity in any ſingle particle; but certain degrees of velocity in a certain number of particles, and that certain number of particles, in a given time is confeſſedly, as the degrees of velocity.

Remark II. The entire effect of theſe preſſures being taken into conſideration, ſeems to overturn this new rule in mechanics for computing the forces of moving bodies, which is, *That the forces are as the quantities of matter, multiplied by the ſquares of the velocities.* And this Mr. *Eames* endeavours to make out thus: The gentlemen who advance this new rule, at the ſame time that they aſſert the velocities in the caſes of the experiment above-mentioned, to be as the ſquare roots of the altitudes do likewiſe acknowledge, that the quantities of the fluid, preſs'd out in equal times, are as thoſe velocities: For, thus an ingenious profeſſor tells us in his *Epitome Element. Phyſ. Math. pars 2da cap. 4, p. 665.*

'the quantities of fluids that issue out of both vessels in equal
'times, are to each other as the velocities; consequently, in
'a subduplicate ratio of the heights of the fluids above the
'orifices.' Now, if this be true, that the quantities of water flowing out in equal times, are as the velocities; then the forces cannot be as the quantities of matter, multiplied by the squares of the velocities: For, then the effects, instead of being proportional, would be more than in proportion to their causes. Thus, the effect of a pressure of a column of any fluid as water, nine inches high, instead of being but nine times greater than that one inch above the orifice, will be no less than 27 times greater: For, the velocity being at this height triple, the quantity of matter in a given time will also be triple; which last multiplied by the square of the velocity gives 27, communicated by a pressure of nine inches in height; while the force communicated by the pressure of one inch, is but as one. So that the moving forces produced will be as 27 to 1; while the causes producing these forces, are but as 9 to 1, *i. e.* 3 times too little for such a purpose.

Thus again, if the velocities be as 1 and 4, the quantities of water issuing out will be as 1 and 4; but the effects, or forces produced, according to the new rule, will be as one and 64; tho' the pressures which communicate them are but as the height, which are as one and 16: Whereas to produce such effects, the height of the latter column ought to have been as 64, *i. e.* four times greater than by experience it is found to be.

Remark III. Mr. *Eames* cannot but observe in the last place, that the common rule of estimating the forces of moving bodies by the quantities of matter, multiplied by their velocities, is rather confirmed by these very experiments: For then, according to the old maxim, effects are proportionable to their causes, the forces communicated will be as the forces communicating, or pressures. Thus let the height, and consequent pressure of any column of water be nine times greater than the height of another; then the velocity of every single particle of water press'd out will be triple; and the number of particles issuing out in a given time will likewise be triple; the forces therefore, resulting from these two multiplied together, according to the common rule will be 9, proportional to the pressure, as it ought to be: So again, if the height be 16 times greater, the velocity will be

quadruple

quadruple, and the number of particles likewise quadruple; and the force produced, the product of these 2, *i. e.* 16 still proportional to the height or pressure.

And universally, the forces communicated, according to the old rule, are in a ratio compounded of two others; one of the quantities of matter, and the other of the velocities: The ratio of the velocities, by the experiments, is the subduplicate ratio of the heights; therefore, the compound of these two is the *ratio integra*, or simple ratio of the heights; in which ratio are the pressures themselves, which produce these moving forces: So that, according to the common rule, the effects are always proportional to their causes.

After the same manner *'SGravesande* reasons in paragraph 355. of *Physices Elem. Math. edit.* 1.

Of the Efficacy *of* Camphire *in Maniacal Disorders*; *by Dr.* David Kinneir. Phil. Trans. N° 400. p. 347.

A Gentlewoman of 19 years of age, from an obstinate fasting for two days, and aversion to see company, in a religious turn before *Easter*, fell into a deep melancholy, would not talk, nor answer any question for some time, but moan'd and sigh'd continually, and slept very little for 10 days: This happened in the decline of the moon: The night before the change she spoke, and call'd for some water to drink; which being given her, she immediately fell a starting and laughing; and her eyes got a briskness in them somewhat uncommon (as her relations told the Dr.) then she began to talk wildly, and continued so all that night: She became next morning very furious; whereupon a physician was call'd, who bled her four times a week the first 14 days, vomited her, purged her, us'd the cold bath, and several other methods common in such cases; but to no purpose. In this condition she continued for nine months, when Dr. *Kinneir* was called.

He first began her with an antimonial vomit, which had no other effect than that of setting her fast asleep for 12 hours: Next day he gave her a drachm and a half of camphire in a bolus, and as much at night: She continued to rest well all that night, and had a great moistness all over her body; and in the day time a plentiful discharge by urine. Thus the Dr. plied her for four days; and afterwards in the day time he ordered her pills of *Æthiops*, *Gum-guaiac*, *Cinnab. Antimon.* and *Pulv. de gutteta*; and at night the dose

of camphire. There were sensible alterations every day for the better; and in three weeks time she recover'd the full use of her reason; and tho' this happened nine years before, she was then in a good state of health, having no extraordinary ailing ever after.

A gentleman, 17 years of age, (from some disappointment) became very silly and stupid: He continued in that way for a long time, notwithstanding he had the best advice: He had a great inclination to chew and eat every thing that came in his way, be it what it would. He rested pretty well of nights, and was pleas'd with the sight of every glaring object. He hid every thing he could lay his hands on, both in company and alone, and was not any wise ill-natur'd. Thus he continued for half a year, when he became so furious, that two men could hardly hold him from beating every body that came about him. Thus he would continue for three or four days together, without sleeping; then after having slept some time, he would become calmer, but was a little mischievous: This was his way from new moon till the full; then he became silly and melancholy, speaking little, and looking always down, not caring to look one in the face: And thus it was alternately for 14 months.

When the Dr. came to the patient, he found him full of complaints of his ill treatment; and had not he had a watchful eye upon him, he had suffered from a blow intended at him; but he got him to take the medicines as before; and with allowing him a more plentiful diet, and a large quantity of diluting drinks, he became perfectly well in six weeks, and he enjoy'd a good state of health and sound judgment for several years, and was then in business.

A mercer's wife, 36 years of age, having born four children, was naturally of a lively, active disposition, fell so ill all of a sudden one day at sermon, that with much ado they could get her out of the church with common decency. She tore every thing about her, talk'd much, and utter'd horrid oaths: In fine she was as much disordered as you can imagine any one. She had the best advice could possibly be given. The physicians fail'd of success, and left off visiting her. About half a year after, Dr. *Kinnier* was consulted about her. He treated her in the foregoing manner; and in four days there was a sensible alteration for the better, and in four days more she went abroad. The Dr. still continued the use of medicines for

14 days

14 days after her recovery; and she was then as well, as ever she was in her life.

A young man, 20 years of age, of a full habit, was so very bad in the sullen, despairing way, wounding himself with his teeth and hands, that there was a necessity of close watching him. The Dr. vomited him twice, and gave him some other things common in such cases; then he began him with the camphire, which in 10 days brought him to reason. He afterwards relaps'd on the change of the moon, was ill for three days, but not so bad as before. He came out of it again, and stood the change of the next moon, with only a little heaviness, as in the hypo. At the height of the same moon, the day before, he was very uneasy, and seemed to resemble a person greatly hypocondriac. The Dr. still continued the use of the camphire, and the other alterative medicines for some time; but in a much smaller dose than what he gave him in his illness: Thus in 9 weeks he was perfectly cur'd, and afterwards continued in good health.

A Method for determining the Geographical Longitude of Places, *from the appearance of falling* Stars; *by Mr.* George Lynn. Phil. Transf. N° 400. p. 351.

UPON perusing Dr. *Halley*'s account in *Phil. Trans.* N° 360. of that extraordinary meteor which appeared all over *England March* 19, 1718-9, Mr. *Lynn* observes one very great use the Dr. suggests might be made of those momentaneous phenomena, in determining the geographical longitude of places, if we could have the least notice of their appearing, &c.

Mr. *Lynn* cannot but think, that some other meteors, which are very frequent, tho' little taken notice of, might serve very well for the same purpose, he means those commonly called *shooting* or *falling stars*, being a sort of natural sky-rockets, discharged at a very considerable height; as he cannot but imagine from this circumstance, namely, that any of them never appears, according to the best of his observation, where the sky is cloudy; and therefore, in all probability, their explosion is in the regions far above the clouds, and they themselves of the same nature with (tho' perhaps less, and much lower than) that great meteor abovementioned, whose height Dr. *Halley* computes to have been above 60 geographical miles, namely, much above the reputed limits of our atmosphere. But supposing these Mr. *Lynn* mentions to be discharged only at 20 or 30 miles high, they may be seen by different observers at the same moment

moment of abſolute time, in very diſtant places from one another, which is the thing required. For, if in any two places, as the Dr. takes notice, any two obſervers by help of pendulum clocks, duly corrected by celeſtial obſervations, do exactly note at what hour, minute and ſecond ſuch a meteor is diſcharged, the difference of thoſe times will be the difference of longitude of the two places; nor does it require ſo much the uſe of a teleſcope, as in the methods hitherto practiſed for that purpoſe.

Now theſe natural ſky-rockets Mr. *Lynn* has found to be very frequent in every ſtar-light night; but eſpecially after a ſtormy day, or in a ſtormy night. If therefore, perſons who are prepared, as above to be exact in their time, and alſo have a moderate knowledge of the ſeveral conſtellations, ſo as to deſcribe the track of any of thoſe meteors among the ſtars, would but beſtow any determinate hour to be agreed on amongſt them; as for inſtance from 8 to 9 o'clock each ſuch night, to watch and obſerve their exploſions, noting down immediately the time and track of them, it would be eaſy to determine upon comparing their obſervations, which of theſe exploſions each of them ſee at the ſame time; and thereby the difference in longitude of thoſe places would be exactly had, as above. It would however be worth the while, to try this way, whether ſuch common meteors are diſcharged, at any conſiderable height above the clouds, and how far, and whether they differ much from one another in their heights.

An Attempt made before the Royal Society *to ſhew how* Damps, *or foul* Air *may be drawn out of any Sorts of* Mines, *&c. by an* Engine *contrived by Dr.* Deſaguliers; *by the ſame.* Phil. Tranſ. N° 400. p. 353.

THE engine conſiſts of a triple *crank*, working three pumps, which both ſuck and force air, by means of three regulators, and are alternately applied to drive air into, or draw it from any place aſſigned, thro' ſquare wooden trunks; which being made of ſlit deal, and 10 inches wide in the inſide, are eaſily portable, and joined to one another without any trouble.

Experiment 1. Dr. *Deſaguliers* filled a tall cylindric glaſs with the ſteams of a burning candle, and burning brimſtone matches, in ſuch a manner that a lighted candle would go out almoſt as ſoon as it was let down into that foul air: Then fixing the trunks or ſquare pipes to the forcing hole of the engine, he drove freſh air into the bottom of the abovementioned receiver;

ceiver: So that the foul ſteam came out at the top of the receiver, which was open.

Exp. 2. Having filled another receiver (cloſe at top) with foul ſteams, as before, he placed it in an almoſt horizontal poſition, only with the cloſe end ſomething above the open end, that the foul ſteam might not go out of itſelf, when ſpecifically lighter than common air. He fixed the trunks to the ſucking hole of the engine; and by working the engine, drew out the foul ſteams from every part of the receiver, as the trunks were applied to them ſucceſſively.

Exp. 3. Having filled the cylindric open receiver with foul ſteams, and ſet it upright, as in the 1. *Exp.* he applied the trunks to the ſucking part of the engine, with their open end near the bottom of the receiver: Then by pumping, the ſteams were all drawn downwards; and ſo out the top of the trunks at the engine; whereas, in the 1. *Exp.* they were driven out at the top of the receiver.

Exp. 4. Having ſet a candle in the cylindric receiver abovementioned, without having filled it with ſteams, and let down the trunks into the receiver, below the flame of the candle, he laid the wet leather over the mouth of the receiver, leaving about ½ an inch open for the air to come in; notwithſtanding this the candle began to dwindle and be ready to go out: But working the engine with the trunks joined to the forcing part, the candle revived, and burned at laſt, as well as in the open air. When he had left off pumping, the flame of the candle diminiſhed again; but when it was ready to go out, it revived again, upon forcing in more air with the engine.

The Dr. makes the following remarks on the abovementioned experiments.

When damps in mines are ſpecifically lighter than common air, they will be driven out of the mine by the firſt experiment.

When damps are ſpecifically heavier than common air, they may be ſuck'd out by the ſecond or third experiment.

When a *ſough*, or *adit*, is carried from a mine to any diſtant valley, to diſcharge the water, or ſave the trouble of raiſing it quite to the top of the pit; ſhafts or perpendicular pits are generally dug from the ſurface of the earth to the ſaid *ſough*, to prevent the workmen from being ſuffocated, as they dig the *ſough*, and that at a great expence: But by the fourth experiment freſh air may be driven down to the workmen, to continue their

their breathing free and safe, and to keep in their candles; by which means the expence of perpendicular shafts will be sav'd.

It has been found by several experiments, that a man may breathe a gallon of air in one minute; and a candle of six in a pound will burn nearly as long in the same quantity of air; the model therefore is capable of supplying fresh air to one man only; and consequently, a large engine will abundantly supply air for the burning of candles, and the working of a great number of men in a mine.

One man may work an engine like the model, and larger every way, in the proportion of a foot to an inch.

As at every stroke 14 cylindric or 11 cubic feet of air are driven in, or as many cubic feet of damp suck'd out, if the axis of the cranks be turned round 60 times in a minute, one man in that time may change the whole air in a cubic space, whose side is 8 feet; and one horse by working 24 pumps with half the velocity, will easily do 4 times the work of one man.

The engines work with a great deal of ease, because no pressure of atmosphere is to be removed; only a velocity to be given to one sort of air, to change it for another.

Fire will not do in all cases, tho' in some it will draw foul air out of mines with success; because several sorts of damps extinguish fire, and some fulminate, and are dangerous, when fire comes near them; and even in common stagnant air, fire will not keep in long.

The Dr. is sensible, that large bellows have sometimes been made use of for this purpose; but they require a much greater power to produce the same effect, and cannot have the advantage of being immediately changed from forcing to sucking; neither are they so cheap as the propos'd engine, which may be all made of wood, excepting the *crank*, which must be of iron, and the barrels of very thin copper.

An Account of the Norwegian Finns, *or* Finlanders; *by Mr.* Peter Kinck. Phil. Transf. N° 400. p. 357.

IN the confines of *Norway* that border on *Sweden*, live a sort of people called *Finns*, whose habitation is in the woods and forests, and who are some of them under the *Danish*, and some under the *Swedish* jurisdiction, of whose original nature, and manner of life Mr. *Kinck* gives some account; and,

1. As to their original, it was from *Swedish Finland* eastward of the sea of *Bothnia*, from whence for want of the neces-

neceſſaries of life, they formerly tranſported themſelves into *Sweden* and *Norway*, where in the foreſts and wilderneſſes they got leave to build and inhabit. For, the common people of *Finland* are ſtrong and hardy; and conſequently, prolific and long liv'd. *Finland* formerly was not ſo well cultivated, as it is at preſent: So that the produce of the earth was not ſufficient to ſubſiſt the numbers of people born there; inſomuch that a great many of them (as the *Goths* and *Vandals*) were obliged to ſeek out for new habitations; and tho' theſe people have for the moſt part retained their native language, yet have they made ſeveral alterations in their manners and ways of living.

As to their language (tho' the much greater part of them both underſtand and ſpeak the *Norwegian* tongue, as well as the *Swediſh*) yet they moſtly uſe their own, which has not the leaſt affinity or reſemblance to either of the other two. As for inſtance, where the *Swedes* and *Danes*, in numbering tell 1, 2, 3, *Een*, *Toe*, *Tree*; they in theirs tell *Yx*, *Kax*, *Kolime*; and as the firſt ſay, for give me bread, *Gig mig brod*, they ſay *Alla mina leip*, &c. And when we ſtand and hear them converſe together, we are ſurprized to conceive how they underſtand each other; ſince they ſpeak ſo very low, that we can ſcarce hear them.

They are generally low of ſtature, but ſtrong, hardy and healthful; their eyes are lively; their noſes high, and their teeth even and white; their feet ſhort. The women are generally of ſo ſtrong a conſtitution, that in child-bearing they ſeldom need any aſſiſtance, and ſoon return to the buſineſs of the family again, except here and there a weakly conſtitution. They are generally ignorant and ſilly; but this muſt be attributed to the little converſe they have with each other, and the reſt of the world; ſince the men, whoſe affairs often call them to traffic with their neighbours, and in different provinces, are of good natural parts, ſharp, and look well to their intereſt. They are frugal, parſimonious, and humble, fearful of giving offence, and very reſpectful to their ſuperiors. They will work whole days without any food, if they can only have tobacco to ſmoak or chew.

Their food chiefly conſiſts in a ſort of fiſh, they call *Oret*, and which anſwers to our ſalmon-trout (it is a delicious fat fiſh, which they catch in the rivers that run thro' the woods) and next to that in bread and flower of rye, which they ſometimes get in great plenty, by cutting down and burning whole foreſts, and ſowing the ground with rye, which ſometimes produces 30 or 40

fold. But this method of producing has been of late very ſtrictly prohibited under very ſevere penalties, by reaſon of the immenſe damage it does, by the loſs of ſo much timber; and becauſe the fire ſometimes lays waſte immenſe tracts of land; So that it is with great difficulty extinguiſhed, burning whole months together, to the great detriment of trade. They frequently uſe bathing at leaſt once a month, thinking thereby they prevent ſickneſs, and diſſipate all weakneſſes from the body; and their method of bathing ſeems ſo very particular, as would ſcarce agree with any other conſtitution, or meet with approbation from phyſicians. The method is thus; in the middle of the houſe (which generally conſiſts of one large room, built all of whole timbers laid acroſs, and notch'd in at the ends to let them cloſe, and then caulked with moſs, as the ſeams of a ſhip are with *okam*) they build an oven with ſtone without mortar, and without a funnel; the ſmoke going out at a hole in the roof, which is left open while the wood is burning in the oven, but ſhut cloſe as ſoon as it is all burnt to a bright coal which keeps in all the heat. When the oven is thus made red hot, they then ſtrip, both men and women, without any reſerve, and place themſelves upon benches made near the roof on purpoſe; then cold water is brought in, which from time to time is ſprinkled upon the oven, whence ariſes a thick ſteam on the bathers, which makes their bodies ſo warm, that they ſweat very plentifully: Each perſon has a rod in his hand, with which they gently beat their whole bodies; and if they find themſelves ſo hot that they cannot well endure it, they call for cold water, which they pour over themſelves in ſo dexterous a manner, that it diffuſes itſelf over their whole bodies, and ſo cools them again. Thus when they have bathed ſufficiently, they go directly out into the air, tho' in the moſt inclement ſeaſon of the year; and what is more will roll themſelves in the ſnow for a good while together, and get no harm by ſo doing: And this method of bathing they make uſe of as their ordinary cure, when they find any indiſpoſition of body upon them.

In the winter when the ground is covered with ſnow, they make uſe of a ſort of long wooden ſhoe three or four elns long, on which they go ſo ſwiftly, that in two hours time they will run 13 or 14 miles; and as they are generally good markſmen, they kill abundance of wild game with their guns both to help to ſupport their families, and ſell to buy themſelves neceſſaries.

They

They are very ignorant about Christianity, by reason of their great distance from any towns; but that misfortune the present king of *Denmark* has taken some measures to remedy: Notwithstanding they seem so ignorant and barbarous, it is very rare that any of them are guilty of any considerable crime.

A preternatural perforation in the upper Part of the Stomach; *by Mr.* Christopher Rawlinson. Phil. Trans. N° 400. p. 361.

ONE *James Skidmore* had complained for three or four years of a violent pain in his stomach and bowels, being never able to rest in his bed at night, till he had vomited up the greatest part of what he had eat or drank the day before The patient compared his pain to some great weight lying upon the region of the stomach, which he in some measure alleviated, by pressing hard with his hand upon that part. When he turned himself in bed from one side to the other, he told Mr. *Rawlinson* he could plainly perceive some fluid fall down with noise to the depending side; which he believed to be the occasion of all his misery: For which reason he often said, he would willingly consent; nay, often earnestly pressed, that the surgeons would cut him open (as he expressed it) and let it out.

The patient had no apparent tumour upon the part, nor was his belly more distended than usual. He had had the advice of several able physicians, before he came into the hospital, but to no purpose. When he died, we opened him to try if we could find out the cause of his complaints. As soon as we had penetrated the *peritonæum*, there flowed out a whitish liquor, not much unlike whey, only a little thicker and more feculent; nor did it emit so noisome a stench, as might be expected from its long continuance in that place. We computed there were upwards of four quarts of this liquor, contained in the cavity of the *abdomen*.

The stomach was found perforated in its upper part, about the middle space between the two orifices, wide enough to contain the tip of one's finger. We cut it open lengthwise, and found it pretty full of a thick glutinous matter, inclining to be yellow; and to its inner coat on the lower side, there firmly adhered the stone of a prune, or some other fruit resembling it: On its inside, near the preternatural perforation, it was gangrened for two or three inches; and on the other side of the perforation there was an ulcer near the same bigness. The

whole ſtomach was a great deal thicker than uſual; but that part next the *pylorus* was above four times thicker than in a natural ſtate. It adhered cloſely to all the parts about it; and it was ſo firmly tied down to the *pancreas*, that it could not be ſeparated without tearing. The ſpleen did not exceed $\frac{1}{4}$ of an ounce in weight. The *pancreas* was ſcirrhous, tho' pretty near its natural ſize. There was no apparent defect in the liver and kidneys; nor had the parts in the *thorax* received any viſible alteration, only that the lungs adhered more firmly to the *pleura* than uſual. The inteſtines, and all the *viſcera*, contained in the *abdomen*, were of a whiter colour than uſual, by being ſo long ſodden in the liquor in which they floated.

A Human Skeleton *of an extraordinary Size found in a Repoſitory at* Repton *in* Derbyſhire; *by Dr.* Degg. Phil. Tranſ. N° 400. p. 363.

REPTON, alias *Repingdon*, is a town on the *Trent*, famous for the burial place of the *Mercian* kings, whoſe chief ſeat was at *Tamworth* in *Staffordſhire*. It is likewiſe remarkable for its free-ſchool, and ancient abbey. After viewing the ruins in this town, and enquiring for antiquities, the inhabitants brought one *Thomas Walker*, a labourer, 88 years of age, who gave the following account.

About 40 years before, cutting hillocks, near the ſurface he diſcovered an old ſtone-wall; and clearing farther he found it to be a ſquare incloſure of 15 foot: It had been covered, but the top was decay'd and fallen in, being only ſupported by wooden joices. In this he found a ſtone-coffin, and with difficulty removing the cover, he obſerved a ſkeleton of a human body nine foot long; and round which lay 100 other human ſkeletons, with their feet pointing to the ſtone coffin: Theſe ſeemed to be of the ordinary ſize. The bottom of this dormitory was paved with broad flat ſtones, and in the wall was a door-caſe, with ſteps to go down to it, whoſe entrance was 40 yards off, nearer the church and river. The ſteps are ſtone, aud much worn. It is in a cloſe on the north ſide of the church, and over this repoſitory grows a ſycamore, planted by the old man, when he filled in the earth. This was atteſted by ſeveral old people, who had likewiſe ſeen and meaſured the ſkeleton.

Instances of long Life; by the Same. Phil. Transf. N° 400. p. 364.

AT *Uttoxeter* in the moorlands of *Staffordshire*, 1722, in 14 days were buried 3 women; the first 103 years of age; the second 126; and the third 87: And the same year were buried two sisters and a brother, of the same parish; the brother was 92 years of age, the youngest sister 95, and the oldest 98.

And in 1726 in the same parish were buried in 22 days the following aged people.

Aug. 7. A woman 94 years of age, and a man	81
Aug. 4. A man aged	68
Aug. 19. A man aged	87
Aug. 22. A man aged	82
Aug. 25. A man aged	83
Men	4
Women	2

In 1726 at *Gravely* in *Hertfordshire*, 31 miles from *London* almost every sixth soul is upwards of 60 years of age. Inhabitants 187, of which there are 29 upwards of 60 years of age.

The Sequel of the Bills *of* Mortality *in* Phil. Transf. N° 380, 381, *for the Years* 1722, 1723, *extracted from the* Acta Breslaviensia; *by Dr.* Sprengell. Phil. Transf. N° 400. p. 365.

The following is a List of those born and buried in the several considerable Towns in Europe *in the Year* 1722.

In *Breslaw* buried.		Christened.	
From the 25th of Dec. to the 31st, 1721.	35	Males	665
		Females	670
January, 1722.	100		
February	94		1335
March	129	Married 424 pair.	
April	121	Among the dead were	
May	122	Married men	231
June	182	Married women	149
July	185	Widows and widowers	150
August	203	Bachelors	57
September	167	Maidens	52

October

October 173 Children to 10 } Boys 570
November 190 years of age } Girls 499
December, till the 24th 90 Stillborn } Boys 53
} Girls 30

: 1791 1791

In *Vienna* buried,
Men, 1038 Among which was, 1 of 105, 1 of 110
Women 942 and another of 111 years of age.
Boys, 1551 Christened.
Girls, 1430 Children 4417

4961

In *Dresden*. Buried 1519. Christened 1514. Married 451 pair.

In *Liepsic*. Buried. Christened.

	Married men.	Married women.	Bachelors.	Maidens.	Boys.	Girls.	Childbed women.	Chrysoms. Males.	Chrysoms. Females.	Stilborn. Males.	Stilborn. Females.	Widowers and Widows.	Summa.	Males.	Females.	Total.	Pairs married.
January	13	6	9	4	20	23	3	4	0	5	3	11	101	44	21	65	[illegible]
February	17	11	11	8	14	7	0	3	3	4	2	13	93	30	26	56	[illegible]
March	10	6	3	4	21	11	3	9	1	6	2	8	84	36	31	67	[illegible]
April	19	9	4	10	17	8	1	3	3	1	2	15	92	39	28	67	[illegible]
May	19	13	13	2	20	14	2	5	2	2	4	15	111	39	41	80	[illegible]
June	11	7	4	2	22	26	0	4	2	5	5	1	89	40	35	75	[illegible]
July	11	8	4	2	24	22	3	4	3	1	2	3	87	40	39	79	[illegible]
August	8	5	6	8	16	9	1	4	3	2	4	2	68	40	34	74	[illegible]
September	8	3	1	1	19	14	1	3	2	1	2	6	61	34	43	77	[illegible]
October	6	9	4	5	12	8	1	6	1	3	6	4	65	49	30	79	[illegible]
November	11	7	3	3	10	14	1	7	0	1	2	5	64	33	38	71	[illegible]
December	11	7	4	4	19	13	0	6	3	4	2	6	79	39	39	78	[illegible]
Summa.	144	91	66	53	214	169	16	58	23	35	36	89	994	463	405	868	[illegible]

So that in this year there died 994, and christened 868. Hence there are 126 fewer born than died.

Among the christened were 2 posthumous, 12 twins, 99 bastards, 1 foundling.

Among the dead were

78	from 60	to 69	years old,
50	70	79	
15	80	98	

In *Weimar*. Buried 143. Christened 190. Among the latter were 107 boys and 83 girls, and among these were 6 twins and 1 bastard. 73 pair were married.

In *Berlin*. Buried 2499. Christened 2701. Among which were 260 bastards. Married 742 pair. So that there were 202 more born than buried.

The following is the special List of the Kingdom of Prussia.

	Christened.	Pair marr.	Buried.
In Angerberg	565	92	212
Balga	604	145	400
Bartenstein	268	42	124
Barthen	309	97	179
Brandenburg	937	223	546
Dutch Elau	157	20	54
Fishausen	473	105	405
Gerdauen	545	92	187
Gilgenburg	205	63	98
Insterburg	2047	381	1013
Johannisburg	382	68	191
Labiau	608	99	280
Liebstadt	443	76	197
Lotzen	263	57	110
Lyck	360	93	119
Marienwerder	791	185	398
Mummel	913	225	521

Neydenburg

Neydenburg	674	153	301
Neuhausen	217	62	141
Neuhoff	37	2	25
Oletzko	726	123	270
Ortelsburg	501	91	260
Osterode	506	97	207
Preuss Elau	490	96	276
—— Holland	666	147	334
—— Marck	142	170	287
Ragnit	803	109	371
Rastenburg	553	118	301
Rein	551	81	174
Schacken	524	130	390
Schonberg	279	66	114
Sehesten	356	76	137
Tapiau	846	176	401
Tilsit	1267	218	632
The cities of Koningsberg	1664	442	1688
Total	20072	4420	11316
In *Cuhrmarck Brandenburg*	18334	4732	11602
Neumarck	7509	1705	4047
The Dukedom of *Magdeburg*	8402	2264	6006
Of *Cleve* and *Marck*	6921	1886	5740
Pomerania	7256	1858	4424
Halberstadt	2899	724	2040
Hohenstein	589	151	403
Minden	2140	479	1351
Ravensberg	2270	605	1600
Moers	491	160	369
Geldern	1686	447	1449
Tecklenburgh	579	118	398
Lingen	729	242	529
Lauenberg and *Butow*	630	156	372
French Colonies	663	130	572
Of all the King of *Prussia*'s dominions } Summa	81770	20077]	[52218

So that there were 29552 more christened than buried. Among the dead were 71 above 90 and 100 years of age; and 1 of 120.

In *Nurenberg* buried.

Married men	194
— Women	233
Bachelors	42
Maidens	61
Boys	285
Girls	230
Stilborn	12
Summa	1057

Born.

Males	501
Females	554
Summa	1055

Among which were 12 twins

In *Copenhagen*	Born.	Married.	Buried.
Males	1345	785 pair	1999
Females	1256		
	2601		

So that there were 602 more born than buried.

In *Amsterdam* died 8421.

In *Epperies* were born 213. Buried 135.
In *Dantzick* were born 2092. Married 490 pair. Buried 1442.
In *Rawitz* were born 200. Buried 74.

The bills of mortality for the year 1723.

In *Breslaw* buried.

From the 25th of Dec. to the 31st, 1722,	22
January, 1723.	118
February	114
March	122
April	102
May	115
June	119

Christened.

Males	711
Females	684
	1395

Married 422 pair.

Among the dead were

Married men	220
Married women	118

July	93	Widows and widowers		150
August	124	Bachelors		48
September	124	Maidens		46
October	85	Children to 10 years	Boys	369
November	114	of age	Girls	306
December, till the 24th	69	Stilborn.	Boys	38
			Girls	26
	1321			1321

In *Vienna*. Buried 5443. Among which were 1079 men, and 974 women, 1758 boys, and 1632 girls. Christened 4457.

In *Lobau*. Buried 171. Christened 226, and 70 pair married.

In *Freyberg*. Buried 321. Christened 362

In *Dresden*. Buried 1654. That is, 165 married men, 136 married women, 36 widowers, and 138 widows, 68 bachelors, 64 maidens, 580 boys, and 467 girls.

Stilborn, boys 54, and girls 46. Christened 1510. That is, 756 boys and 754 girls. Among which were bastards 66 boys and 41 girls. Married 396 pair.

In *Liepsick*. Buried 928. Among which were 126 married men, and 88 married women, 53 bachelors and 36 maidens, 199 boys and 164 girls, 21 women in childbed, newborn boys 60, and girls 46, besides stilborn boys 37, and girls 30; also 68 widows and widowers, Christened 966; as 489 boys and 477 girls. Among which were 11 pair of twins and 99 bastards. Married 306 pair.

In *Erfurt*. Buried 448. Christened 666.

In the Elector of *Mentz*'s dominions in 75 villages are buried 543, christened 872.

In *Berlin*. Buried 2618. Christened 2770. Among which 289 bastards.

In the towns and villages of *Old*, *Middle*, and *Uckermark* are christened 19058. Among which are 995 bastards. There were married 4943 pair, and buried 13317. Among which were 23 who lived to 90, 100, and 1 to 112 years old. So that there were 5741 more born than buried.

In the rest of the King of *Prussia*'s dominions.

	Born.	*Married*	*Buried.*
In the kingdom of *Prussia*	21384	4957	10762
In the elect. of *Brandenburgh*	18774	4863	13015
In *Neumark*	6907	1813	4193
In the dutchy of *Magdeburg* and county of			
Mansfield	8858	1781	5579
Cleve and *Mark*	6645	1924	6848
Pommern	8544	2241	5294
Halberstad	2943	808	2162
Hohenstein	604	165	389
Minden	2087	565	1925
Ravensberg	2061	613	1833
Moers	484	156	371
Geldern	1866	436	1478
Tecklenburg	475	141	437
Lingen	701	270	641
Lauenberg and *Butow*	588	149	340
French Colonies	594	177	563
	83515	21059	55830

Among the born are 2157 bastards, and among the buried 77 that were above 90 and 100 years of age. So that there were 27685 more born than buried.

In *Copenhagen* were born 2604. Buried 1914. Married 701 pair. Therefore 690 more born than buried.

In *Amsterdam* there died 7119, and were married 2289 pair

In *Epperies*, born 203. Died 132.

In *Dantzick*, Born 2002. Buried 1495, and married 537 pair.

A vast Quantity of purulent Matter running out of the Mouth of a Boy, and causing a rottenness of the Cheek-bone; *by Dr.* Hardisway. Phil. Trans. N° 400. p. 374 *Translated from the* Latin.

FEB. 16. 1724. Dr. *Hardisway* was called to see a boy of 10 years of age, who for six weeks was afflicted with a lingering fever: After prescribing the proper remedies, he recovered in 6 or 7 days: But some time after, there arose a large tumour on the inside of his right cheek, the boy being otherwise very well in health. Upon opening his mouth, tho' with some difficulty, there came to view a large quantity of *pus*; but whence it proceeded the Dr. could not then discover, on account of the boy's untowardness; he therefore, only ordered to apply a fig boiled in milk, to soften, and if possible, to break the tumour. Next morning there issued out a vast quantity of *pus* from his mouth (the tumour in the mean time breaking) of a very noisome stench. And after prescribing a moderately deterging gargarism, there run out about the evening the same quantity as before. The Dr. was very much surprised whence this *pus* could proceed, since the ulcer on the cheek did not at all seem sufficient to afford such a quantity: He suspected (especially on account of the stench) that the source of this matter lay somewhere in the head within the bones; and upon a more narrow survey, he found the bone of the upper jaw to the right entirely bared of its flesh (which, as if half boiled, parted asunder) and carious; and a large quantity of purulent matter discharged from the sockets of the teeth, which last the boy himself could now with his fingers easily pull out: On which account a surgeon was called for; and the Dr. advised him, while he endeavoured the cure of the bone, at the same time to deterge, and consolidate the ulcer; but notwithstanding all the Dr. could say, the surgeon would apply nothing to the suppurated cheek: Whence it was, that it again became of a prodigious size, and soon grew twice as big as the other. And then he only applied to the carious mandible a tent, dipped in some tincture or other, possibly that of myrrh. But finding his tincture of little or no service, he at length entirely extracted the *os malæ*. Upon which neither did the cheek subside, but rather increase, nor the quantity of *pus* diminish. With this bone was taken away the *os palati* and the *septum* of the nose, as also the lowermost part of the orbit of the eye, as the Dr. has endeavoured to represent more plainly in the annexed figures:

figures: Add to this, that the tumour, lying upon the remaining *processus zygomaticus*, makes this cheek twice bigger than the left, and almost shuts the eye, the *pus* continually flowing from a lasting spring, as it were.

Fig. 5. Plate VI. represents the outside of the bone; *a a a a a* the *alveoli* of the teeth; *b* the farther grinder; *c* part of the orbit of the eye; *d* part of the bone, where broken off near the nose; *e* part of the bone where the *processus zygomaticus* terminates; *f* part of the bone under the upper lip.

Fig. 6. represents the inside of the bone, 1. The part broken off at the nose; 2. The exterior part of the mandible and the *alveoli* of the teeth; 4. That part of the bone that reaches the eye; 5. The farther part of the jaw; 6. Part of the right nostril, with the *septum* broken off; 7. The grinder.

Fig. 7. represents the bone in an upward view; DDDD part of the *os palati*, EEE the *alveoli* of the teeth; FF the exterior part of the mandible; G its anterior part; H the grinder.

Fig. 8. represents the inside of the bone somewhat reclined; AAAA a part of the mandible and the *alveoli* of the teeth; B the place, where the bone was broken off near the nose; C the part of the bone that reached the eye; D the farther part of the jaw; E the farther grinder; F part of the *Os palati*; G part of the right nostril with the *septum* broken off; as in N° 6. Fig. 6.

After this, the Dr. acquainted the editor by a letter, dated *Dec.* 24. 1727, that the above described tumour continued in the same state, a purulent matter flowing from the mouth and jaws, as usual; that the boy was in other respects in health, and that he eat, drank, slept, and play'd cheerfully every day with his other companions.

The Dissection of the poisonous Apparatus *of a* Rattle-snake, *with an Account of the quick Effects of its* Poison; *by Mr.* John Ranby. Phil. Trans. N° 401. p. 377.

THIS rattle-snake was sent from *Virginia*, and delivered to Mr. *Ranby*, on purpose to make such experiments with it, as might inform mankind of the symptoms, which attend its bite, and the appearances in the dead bodies of such animals, as have been bit by it. It is only by this method, and a number of facts faithfully stated, and compared with each other, that we may hope one time or other to discover the manner of the poison's operating, and perhaps to find out some remedies,

either

either internal or external, to relieve perſons bit by it. The anatomy of the rattle-ſnake having been ſo accurately deſcribed by the ingenious Dr. *Tyſon*, very little more can be added to his account: Mr. *Ranby* therefore, only takes notice of the inſtruments of its poiſon, ſome of which are different from what that celebrated anatomiſt obſerved.

Upon removing the common integuments of the head, the muſcles that raiſe the poiſonous fangs appear; the firſt of which ariſes with a ſhort fleſhy beginning from the upper edge of the lower jaw, near the articulation of one of thoſe bones, which Dr. *Tyſon* calls *maxillarum dilatatores*, as repreſented at A Fi.1, Plate VII. and ſends a few carnous fibres to the ſide of the *cranium*; then becomes tendinous, and ſo tends to its inſertion in the outſide of the bone, which receives the poiſonous fang (Fig. 2.)

Upon removing this muſcle there appeared a gland, repreſented at B Fig. 1. about the bigneſs of a ſmall pea, which Mr. *Ranby* takes to be one of the maxillary glands, for the following reaſons.

1. The ſtructure of the parts, and diſtance from the fang, make it unlikely to be deſigned for ſeparating the poiſonous fluid, but rather a *ſaliva* to moiſten the *aliment*, in order to make it paſs down the *Oeſophagus* with eaſe, the ſtomach of thoſe animals being but ſmall, and the gullet conſiderably larger; not without ſome analogy to the *ingluvies*, or crop of granivorous fowls, where the food ſtops for ſome time, and is moiſtened, before it can deſcend into the ſtomach.

2. Theſe parts are contrived in ſuch a manner, that upon opening the mouth to receive the prey (at which time ſuch a fluid is moſt wanted) the muſcle abovementioned, preſſing on the gland, promotes the diſcharge of its contents into the mouth: The duct of this gland ſeems to open between the upper lip and the jaw; but as the excretory ducts of ſo ſmall a gland are rarely to be ſeen with certainty, Mr. *Ranby* does not pretend exactly to determine its aperture. Under this gland lies another muſcle ſmaller than the former, which ariſes, and is inſerted near it, as repreſented at C Fig. 1. Theſe two muſcles draw the bone D, in which the poiſonous fang is fixed a little outwards and upwards. Between the laſt deſcribed muſcle and gland paſſes a nerve to the upper part of the bone, which receives the tooth E Fig. 1. and B Fig. 2. and it is probable that this nerve has been taken for the excretory duct of the gland before mentioned.

Upon

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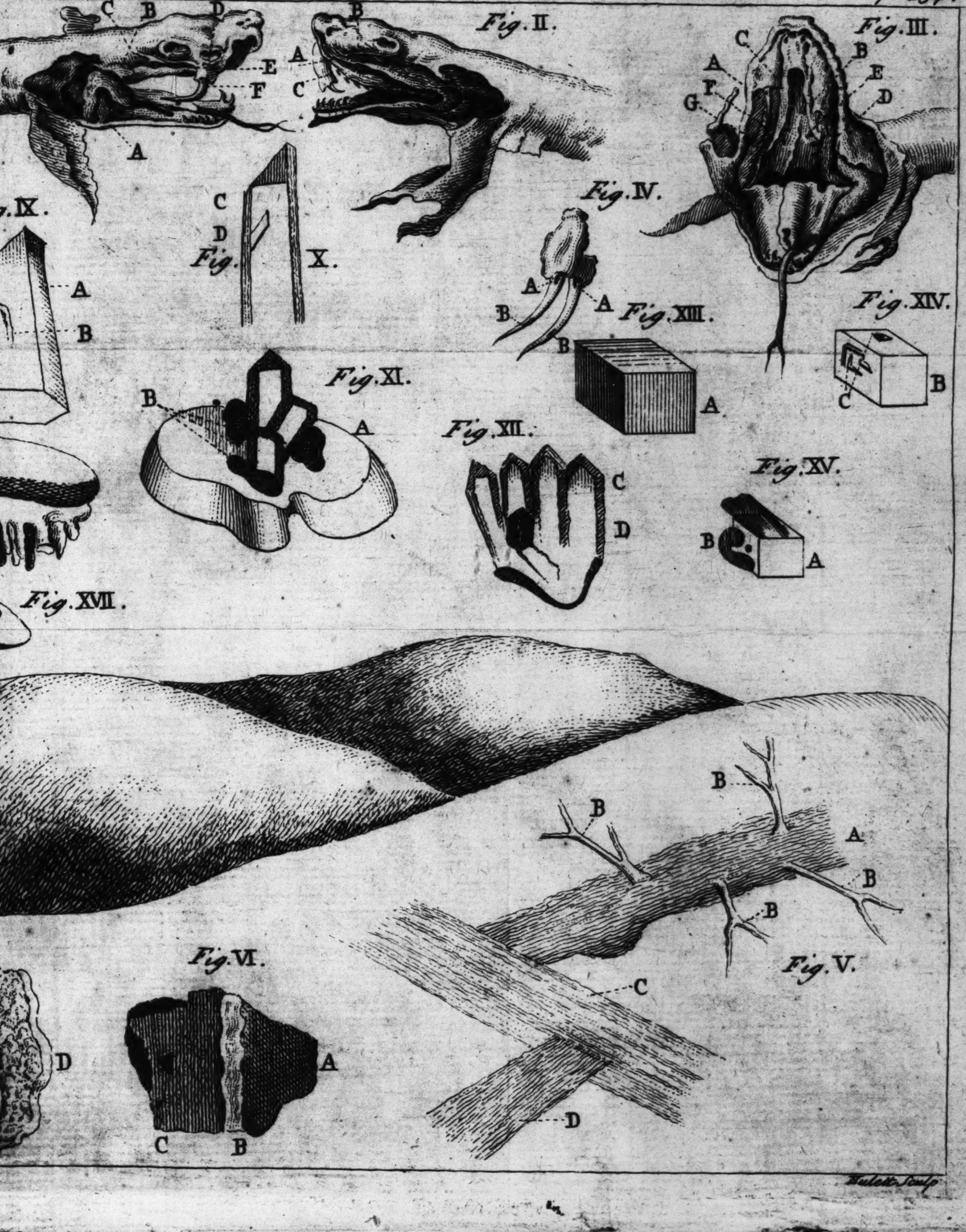
PLATE. VII.

p. 254.



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Upon opening the mouth, two ſmall eminencies appear in the fore-part on the inſide of the upper-jaw, being a membrane, raiſed by the fangs and drawn over them, like the mouth of a purſe, as repreſented at A B Fig. 3. and at C Fig. 2. This membrane is thick and ſtrong; and viewed in a microſcope, appears to have a number of glands, ſome of which are even viſible to the naked eye. In a common viper he obſerved one on each ſide the fang: Theſe membranes prevent the involuntary diſcharge of the poiſon out of the fangs (which in his opinion are the only repoſitories of that fluid) into the mouth, as alſo killing with the fangs little animals on which they ſometimes feed. Upon putting back this membrane, the fatal fangs appear, which at firſt ſight ſeemed to be only one on each ſide, till ſearching farther there appeared four more, the firſt and largeſt is fixed in a bone, which is articulated to the fore part of the upper jaw, as at F Fig. 1. The other four are faſtened in and covered with ſtrong tendinous membranes, and lie one over another, as it were, as at B Fig. 2; C and E Fig. 3. Theſe teeth are crooked and bent, as repreſented in Fig. 4. eſpecially the firſt, and have each two perforations, the one on the upper part, the other on the lower part of its convex ſide; which laſt comes quite to the point, and reſembles the ſloping cut of a pen. The upper perforation, as repreſented at A Fig. 4. Mr. *Ranby* imagines to receive the poiſon, the other tranſmiting it into the wound, B Fig. 4. All theſe fangs are tubular, the largeſt of which contained a ſmall quantity of a tranſparent fluid of a light yellowiſh colour, which, upon putting the ſnake into ſpirits of wine, changed to a beautiful red: The fangs of the common vipers, Mr. *Ranby* examined, had the lower perforation nearer the middle. Upon removing the membrane out of the mouth, a muſcle appears about the ſize of the firſt deſcribed above, which ariſes from the middle of the *Dilatatores maxillarum*, D D Fig. 3. and is inſerted in the under ſide of the largeſt tooth; for, the force required to pull down the fang being leſs than to raiſe it, fewer muſcles are required. This animal was in Mr. *Ranby*'s cuſtody about a month, during which time he bit three dogs, and a cat, the firſt of the dogs (bit at the College of Phyſicians) died about two minutes after the bite, and the moment he was bit he became convuls'd, and loſt the uſe of his limbs. The wounds were exceedingly ſmall, and between the pectoral muſcles. Upon opening the dog, the ſkin and *membrana adipoſa* for the breadth of a crown-piece were livid, about the wound; as if from a violent blow. The

ſecond

second dog had the same symptoms with the first, but lived near a quarter of an hour, and had bloody stools. Three days after, Mr. *Ranby* carried the snake to bite another dog and cat: The dog was larger than either of the two former, and having been bit at the extremity of the nose he was immediately affected, howl'd, shook, fell down and foam'd at the mouth; and in about 10 minutes involuntarily voided his *fæces*, tinged with blood: He died in about two hours. The next day Mr. *Ranby* opened the body, and observed the abdominal contents very much inflamed, especially the stomach and intestines, which appeared almost equal to the finest injection: Upon opening the stomach and intestines they contained a mucous matter, the greatest part of which was blood, and the fine villous coat, so visible in these animals was entirely destroyed. About an hour before he was bit he had a plentiful meal of coarse beef, of which there was not the least appearance. Upon opening the *thorax*, the *pleura* and other membranes looked as if injected; the heart was turgid with blood, as were also its vessels. The vessels of the membranes of the brain form'd a most beautiful figure from the quantity of blood contained in them, as did likewise the blood-vessels of the nerves; there was a small quantity of water between the two hemispheres. The blood contained in the heart and its vessels, was an even mass, about the consistence of cream. The cat had (upon opening) nearly the same appearances, and lived about five hours.

Of the Proportion of Velocity and Forces in moving Bodies; *by Dr.* Samuel Clarke. Phil. Trans. N° 401. p. 382.

EVERY effect must necessarily be proportionate to the cause of that effect; that is to the action of the cause, or the power exerted at the time when the effect is produced. To suppose any effect to be proportional to the square, or cube of its cause, is to suppose that an effect arises partly from its cause, and partly from nothing.

In a moving body, the force arising from the quantity of the matter, as its cause, must necessarily be proportional to the quantity of the matter; and the force arising from the velocity of the motion, as its cause, must necessarily be proportional to the velocity of the motion: The whole force, therefore, arising from these two causes, must necessarily be proportional to these two causes taken together; and, therefore, in bodies of equal bigness and density; or in one and the same body, the quantity of

of matter continuing always the ſame, the force muſt neceſſarily be always proportional to the velocity of the motion. If the force were as the ſquare of the velocity, all that part of the force, which was above the proportion of the velocity, would ariſe either out of nothing, or (according to M. *Leibnitz*'s philoſophy) out of ſome living ſoul, eſſentially belonging to every particle of matter.

Whenever any effect is in a duplicate ratio, or as the ſquare of any cauſe; it is always, becauſe there are two cauſes, acting at the ſame time, or that one and the ſame cauſe continues to act for a double quantity of time.

The reſiſtance, made to a body moving in any fluid medium, is in a duplicate ratio to the velocity of its motion; becauſe in proportion to its velocity, it is reſiſted by a greater number of particles in the ſame time. And again, in proportion to its velocity, it is reſiſted by the ſame particles ſingly with a greater force, as being to be moved out of their places with greater velocity.

Light decreaſes in a duplicate ratio of its diſtance from the ſun; becauſe the rays divaricate according to two dimenſions, namely upwards or downwards, and ſidewiſe: But according to the third dimenſion forwards from the ſun, a ray of light undergoes no alteration; becauſe the particles of which it conſiſts, being all emitted with an equal velocity, continue every where at equal diſtance from each other.

One and the ſame cauſe, acting in a double quantity of time, produces the ſame effect, as two equal cauſes acting in a ſingle quantity of time. One and the ſame force in two parts of time will cauſe a body in motion to deſcribe the ſame ſpace, as double the force would do in one part of time: The ſpace, therefore, deſcribed by a body in motion, is not as the force; but as the force and the time taken together. A body, with any the leaſt aſſignable force, will move thro' infinite ſpace, if it meet with no reſiſtence in an infinite time: And in ſpaces, where there is an uniform reſiſtence to motion, the ſpace, deſcribed before the motion ceaſes, muſt needs be as the force, and as the time together: Becauſe a double force will carry a body twice as far in the ſame time, and will alſo cauſe the motion to be twice as long time in deſtroying by an uniform reſiſtence: The ſpace, therefore, deſcribed before the motion ceaſes, is in this caſe, demonſtrably, as the ſquare of the force. A body, thrown upwards with double force, will be carried four times as high, before its motion be ſtopped by the uniform reſiſtence of gravity;

becauſe

becauſe the double force will carry it twice as high in the ſame time; and moreover, require twice the time for the uniform reſiſtence to deſtroy the motion. The caſe is the ſame in accelerated motion; in bodies accelerated by a ſucceſſion of elaſtic impreſſions, or falling with a motion accelerated by the uniform power of gravity, or by any other uniform power whatſoever. The ſpace deſcribed muſt needs be as the force, and as the time, wherein the force operates.

What Dr. *Clarke* has thus demonſtrated concerning any force, conſidered as the cauſe, producing an effect, and concerning the time, during which the force operates, is on all hands acknowledged to be true concerning velocity: And, therefore, velocity and force in this caſe are one and the ſame thing: So that to affirm force to be as the ſquare of the velocity, is to affirm that the force is equal to the ſquare of itſelf.

Now from hence appears very clearly the ground of the error theſe Gentlemen have fallen into, and of their miſapplication of the experiments they build upon.

The effect of a force, impreſſed on a moveable body, is the motion of that body from one place to another. Now in regard that the effect muſt be proportional to its cauſe, hence M. *Leibnitz* (whom the other Gentlemen have followed) contends that the ſpace, deſcribed by a body in falling, is proportional to the force by which it is impelled during its fall; which ſpace being agreed to be as the ſquare of the velocity (as being proportional to the velocity and time taken together) hence they infer, that the force likewiſe is as the ſquare of the velocity.

But from what has been ſaid, it is plain, that the ſpace, deſcribed in theſe, and all other the like caſes, is not as the force only, but as the force and as the time, wherein the force acts: that is to ſay, as the ſquare of the force: For, the cauſe of the quantity of the ſpace deſcribed, is not barely the quantity of the force; but alſo the continuance of the time wherein the force acts. The force, therefore, and the time, taken together being neceſſarily as the ſpace deſcribed, as the velocity and the time, taken together, are on all hands acknowledged to be; it follows, that the velocity and the force are equal, and not the force as the ſquare of the velocity.

When two unequal bodies, faſtened at the ends of the arms of a balance of unequal length, counterpoiſe each other, and vibrate in equal times, as they muſt neceſſarily do, being faſtened to the arms of the ſame balance; which is an obſervation M. *Leibnitz* lays great ſtreſs upon: In that caſe, it is true, the force

forces will be as the ſpaces deſcribed; but, not therefore, as the ſquare of the velocities. For, in that caſe the velocities themſelves are as the ſpaces deſcribed, becauſe the times are equal.

When a body, projected with a double velocity, enters deeper into ſnow, or ſoft clay, or into a heap of ſpringy or elaſtic parts, than in proportion to its velocity; it is not becauſe the force is more than proportional to the velocity; but becauſe the depth, it penetrates into a ſoft medium, ariſes partly from the degree of the force, or velocity, and partly from the time wherein the force operates before it be ſpent.

In the colliſion of hard bodies; it is (the Dr. thinks) agreed on all hands, that it is demonſtrated by reaſon, and confirmed by experience, that when a perfectly hard ball, moved with whatever degrees of velocity, ſtrikes full upon another hard ball, equal in bigneſs and weight, and without any motion in it; if the balls be unelaſtic, they will both go on together the ſame way, dividing the motion equally between them, with half the velocity the firſt ball originally had: But if they be perfectly elaſtic, the moving ball will communicate its whole motion and velocity to the quieſcent ball, and itſelf lie ſtill in the other's place. Were it true now, that the force of the moving ball was as the ſquare of its velocity; theſe experiments would then ſhew (which is infinitely abſurd) that the force, or *vis inertiæ* in the quieſcent ball, namely the dead force, was always proportional to the ſquare of the velocity (which theſe Gentlemen affect to call the living force) of the moving ball, whatever its velocity were: Or the force in both might juſt as reaſonably be ſuppoſed to be as the cube, or the quadrato-quadrate, or any other power of the velocity of the moving ball; which is turning the nature of things into ridicule.

The Variation of the Compaſs *at* Vera Cruz *in* 1726, 1727; *by Mr.* J. Harris. Phil. Tranſ. N° 401. p. 389.

IN 1726, 1727, Mr. *Harris* ſeveral times obſerved the magnetic variation at *Vera Cruz*; and found it to be 2 degrees and $\frac{1}{4}$ eaſterly.

He alſo ſeveral times obſerved the variation on his voyage from *England* towards *Vera Cruz* (having on board a good azimuth compaſs) but Mr. *Harris* always found, that the beſt obſervations he could make, when compared together, differed ſo much, that he could not depend upon them, to much leſs than 3 or 4 degrees, or ſometimes half a point of the compaſs.

A new Method for composing a Natural History of Meteors; *by Mr.* Isaac Greenwood. Phil. Transf. N° 401. p. 390.

THIS method in general is, that in addition to such observations, as should be made on land, there might also be some account taken of those made at sea, which already are by far more numerous than what were ever made ashore, or indeed, what can be expected thence for some ages still to come. This method occurr'd to Mr. *Greenwood*, as he was looking over various journals of voyages in his passage from *England*, in which he was not a little surprised to find the following particulars constantly observed.

1. There was a general account of the weather for every day, during the passage of the ship on the voyage, which tho' not quite so exact, as the observations of the same kind, that have been made on land, particularly what were published by Mr. *Derham*; yet for all that he knows, are sufficient for the design. However, if there is any defect in this article, it is abundantly made up in another column; which is a far more exact register of the direction of the winds than was ever kept ashore, being an account thereof to every two hours in the day. This article may, perhaps, be of very great importance; since as Dr. *Jurin* observes; 'it is manifest, that the sudden changes 'of the weather are chiefly owing to the winds.' As to the degree or strength of the wind there are also sufficient *data* in all sea journals to determine it, as shall be particularly shewn anon: Lastly there is a daily account inserted of the latitude and longitude of the ship; so that there will be no difficulty in computing what part of the globe each observation belongs to.

And now since there is in the world a great variety of these marine observations, already made (for, in all voyages whatever that have been performed for several years past, it has been customary to keep an exact journal of the aforesaid articles) Mr. *Greenwood* thought it might be no difficult matter to collect therefrom the history of the winds and weather in most parts of the ocean.

In order to this he imagined that if the *Royal Societies* of *London* and *Paris* should encourage such a design, they might easily procure extracts from most of the journals, kept in their respective nations: For, certainly such Gentlemen, as would be at the pains to keep a constant diary of the weather, could not fail

fail alſo to communicate ſuch marine obſervations, as they ſhould be able to obtain.

The ſeamen likewiſe themſelves (among whom there is a conſiderable number of ſuch, as have a taſte for phyſical knowledge) as they are under a kind of neceſſity to obſerve the winds exactly, &c. would not be backward in tranſmitting their obſervations; eſpecially, when they were informed of what importance and advantage it might be to themſelves, and the cauſe of navigation.

Mr. *Greenwood* proceeded farther to think, that if the aforeſaid *Societies* ſhould judge it improper to be at ſo great an expence, as would be requiſite in printing ſo many extracts from ſuch journals, as ſhould be ſent to them, that they might notwithſtanding keep in manuſcript a book of tables of ſuch marine obſervations, as they ſhould think fit to collect therefrom; and that the ſecretaries of the *Society* ſhould take care, that all ſuch obſervations were tranſcribed in their proper places.

The

The form of theſe tables he thought might be in the following manner.

	January the 1. 1726.									
Latitude	Longitude 20				21			22		
	H	W	D	Weather	W	D	Weather	W	D	Weather
	12	N	2	fair						
	6									
	12	N b E	3	fair						
	6									
30	12							S b E	2	cloudy
	6								2	rain
	12							S	3	ſtorm of rain
31	6							S b E	2	rain

In which the title ſhews the year, month and day; the horizontal ſpace juſt below it, the longitudes; the vertical ſpace, without the double lines, the latitudes; that within the double lines, the hour of the day; and the horizontal ſpaces under the longitudes, the wind, its degree or ſtrength, and the weather, which are accordingly mark'd with W. D. Weather.

In this ſpecimen Mr. *Greenwood* has noted every degree of latitude and longitude, that the work might be the more perfect. He has only taken notice of four hours in the day, *viz.* 12 at noon, 6 in the afternoon, 12 at night, and 6 in the morning. However, if there be required a greater exactneſs in this article, it will be eaſy enough to frame tables accordingly. He began the hours with 12 at noon, becauſe all journals are kept from that period, the marine day being always counted from noon to noon. There may be other columns inſerted, as ſhall be mentioned anon; tho' what he has already taken notice of is ſufficient to the preſent deſign.

Of theſe ſort of tables there muſt be at leaſt four volumes; one for that part of the *Atlantic* ocean, which ſuch ſhips generally paſs over, that trade between *Great Britain* and the *Weſt Indies*; another for thoſe parts of the ocean, that lie in the paſſage of ſuch ſhips, as are employ'd in the *Mediterranean* or *Turky* trade; to which may be added a table for the *African*, and *Indian* commerce; a third may be fram'd for that part of the ocean, that lies between the northern provinces in *America* and the *Weſt Indies*; and a fourth for the ſhips that paſs between *New England* and *New York*, and *Britain*, which on the northern part may be made ſo wide, as to take in the *Newfoundland* trade, &c.

It muſt be confeſſed, that the work will be very much protracted, and require ſome conſiderable application and care, in extracting ſuch obſervations from journals, as ſhall be of uſe. There will alſo be ſome difficulty in procuring any conſiderable number of ſuch journals; and laſtly, there is but a very ſmall number of obſervations made in compariſon to the ſpaces that muſt be allow'd in the tables for them, by which means there muſt neceſſarily be a conſiderable waſte.

In anſwer to theſe objections, it may be ſaid in general, that there will be much leſs application and care requir'd than

than in keeping a diary of the weather, *&c.* on the land: By this means likewise, there may be more observations collected in a few years than can be expected from the other method in some ages; and one man may in a few months hereby compile a larger history of the weather, than what has hitherto been done by the united observations of all such, as have undertaken this province.

Tho' there might be some difficulty as to particular persons in procuring a great number of journals; it cannot be suppos'd, that the Royal Societies at *London* and *Paris* should meet with the same. It is also observable, that in the Royal Navy of *Great Britain*, the masters of the mathematics are obliged to keep such a journal by an Act *Geo.* I. on board every ship, which without doubt might be easily obtain'd on this occasion: Nor can we imagine that any in the trading interest would refuse a thing, that tended so much to their own advantage and benefit.

It is true, there can be no remedy for the many empty spaces in the tables (if that method be followed Mr. *Greenwood* has propos'd) however, this will be look'd upon as a trifling objection, by such as consult the improvement of natural knowledge rather than the waste of paper.

Mr. *Greenwood* concludes these general remarks, by observing, that as the history of the winds and weather is capable of a more speedy and expeditious improvement from marine observations than from diaries from the land. So likewise it is capable hereby of a more large and extensive improvement. It will, without doubt, require several years, before observatories of the weather, *&c.* will be erected at the universities, and capital towns of the provinces, shires, *&c.* in *Europe* (if ever such should be) not to mention *Africa*, *Asia*, and *America*, from which little can be expected in this matter; and yet upon that supposition, how few would the diaries be in comparison of the great number of journals that are annually kept at sea: Besides many thousands that might perhaps be obtain'd, relating to the course of the winds and the weather, successively for several years last past. Mr. *Greenwood*, it is true, cannot calculate with any exactness, the number of vessels there may be upon the seas he has mentioned in the space of one year, and consequently, how many distinct journals there are annually kept: However, if Mr. *Greenwood* might judge from the trade of the little town he was in at that time, viz. *Cambridge* in *New England*, there

there must be several thousands: For, there are seldom less than 8 or 900 voyages made to and from this port in a year. Mr. *Greenwood* only adds in this place, that the method here propos'd seems to have the advantage of the common method, heretofore us'd in composing the natural history of meteors; in as much as that requires a particular application and attention, without any other views, and advantages; whereas in this case there is a kind of necessity of making such observations, in order to conduct a ship safely thro' the ocean, whether the philosophical part of mankind shall think fit to improve them in their interest, or no: However, Mr. *Greenwood* would not be understood by any thing that has been said upon this head, to derogate from the design of observing on land; for, that has likewise several advantages, that can by no means be pretended to in the new method.

We may be able from this method to define with a great deal of exactness the bounds and limits of all considerable winds: For, as there are at all times in the year some hundreds of vessels at sea, it is of the same importance in our case, as tho' there were so many distant observations there; and that the knowledge of these more extensive and general winds, would be of considerable use, none will deny, that shall attentively consider it; for, hereby we may be able to judge, in what place such a wind has its origin, for how long a time it continues, with what velocity it moves, where its greatest strength is, and how great a tract of the earth it passes over. By this means likewise, we may, perhaps, in process of time, arrive to so much skill, as to judge whether some considerable certainty, from the rise or beginning of a wind, what its effect and issue shall be, which will be of as great importance in navigation, as any thing that is still wanting. Again, from such marine observations of the more extensive and lasting winds, it is not impossible, that we should be able to form a probable judgment of the effect and influence of the wind upon the weather; which, for what cause he knows not, he had frequently observ'd at sea to change and alter, according as that did.

From collecting all such meteorological observations as are made at sea, we may reasonably expect to come to the knowledge of such winds, as prevail most in particular latitudes. Tho' the wind be a very uncertain meteor, there is no doubt, but that in some places, it has a very different course from what it has in others, And if Mr. *Greenwood* mistake not,

he thinks it has alſo been frequently obſerv'd, in ſome particular places, that the courſe of the wind in one year has been much the ſame as in others; and tho' there has been no particular order, or exactneſs, hitherto diſcover'd; yet the prevalent winds, or the greater number of winds have been in both caſes, according to the ſame direction: In theſe parts of the world it is remarkably ſo. We cannot, it is true, expect to diſcover the reigning or prevalent winds of ſuch latitudes, as are very diſtant from the tropics, by as eaſy an obſervation, as the trade winds, and *Monſoons*, which are in the torrid zone, were firſt found out. However, as it has been after ſeveral obſervations, that the courſe of thoſe fix'd winds was determined, we may alſo hope that time and induſtry may bring us to a much better knowledge than what we have at preſent of theſe that are more variable. It is needleſs to ſay of how much importance it would be to the trading part of the world, were we able to define the more frequent and reigning winds of every climate; for, as the probability of voyages might then be calculated in the ſame manner, as that of other chances, the ſailor might then better know how to order his courſe, in ſuch a manner, as to arrive wirh the moſt probable diſpatch to his port.

It may alſo not be impoſſible, from a protracted series of ſea obſervations, not only to know the general courſe of the winds in every climate throughout the whole year; but alſo to form a very probable judgment of the reigning winds of the ſeveral ſeaſons of the year, and probably, of every month too: Which if it could be once obtain'd, we ſhould have nothing more uncertain in navigation, than that it was a doctrine of chances, which might be mathematically calculated.

Mr. *Greenwood* adds under this head, but one thing more which we may with all the probability imaginable expect to arrive to, *viz.* the particular ſeaſons, ſigns, and places of the tornado's and hurricanes. The effect of theſe are in many caſes ſo fatal, that they call for all our ſkill and obſervation: And could the hiſtory hereof be ſo ſucceſſively known as that we might be able to draw any certain concluſions from it relating hereunto; it might, perhaps, be a ſufficient recompence for all the care that is requir'd, in the whole collection of marine obſervations.

Mr. *Greenwood* thinks he might add in common to the two foregoing heads, that the marine obſervations have much

the advantage of ſuch as are made on land (which notwithſtanding are of very great ſervice) in regard they are not liable to any external accidents, as theſe are; the winds on ſhore being frequently interrupted in their courſe, and often diverted therefrom, by interveening mountains, vallies, or promontories.

Were we to reckon among the advantages of this new method of obſerving on the winds and weather, thoſe incidental obſervations that might be found in journals, of general benefit to mankind, they, perhaps, are alone ſufficient to engage us in the undertaking: He only hints here, that if it ſhould be thought proper to put in practice his deſign, it may, perhaps, be worth the while to inſert into the meteorological tables, ſuch obſervations as relate to the variation of the compaſs and currents; the true knowledge of which would be of no inconſiderable ſervice to navigation.

If there was likewiſe a column left for ſuch remarkable accidents as did occur, it might not be amiſs; particularly, any uncommon diſcoveries of lands, rocks, or ſoundings, exceſſive thunder and lightening, &c. luminous appearances in the ſky; and what remarks may be found to relate to the water-ſpout, which, tho' perhaps one of the moſt curious phenomena of nature, is as little known as any whatſoever; ſubmarine *hiatus*'s or whirlpools, if any ſuch there be; and laſtly, any extraordinary rendezvous of fiſh, &c. that are us'd in the affairs of life; not to mention ſuch deſcriptions, as may relate to matters of mere ſpeculation and curioſity.

But theſe ſorts of accidental advantages, in ſuch a collection of journals of voyages, as is neceſſary to this deſign, are too numerous to be inſiſted on; he, therefore, only adds one more, namely, the great improvement there would hereby be given to geography, a ſcience of the greateſt uſe and importance in the affairs of life: Not only all hydrographical charts might by this means be corrected, and brought to the truth; which is of ſo much concern, that the lives of a great part of ſuch as go to ſea depend upon it; but alſo the diſtances and ſituation of all ſea-ports, and ſeveral other things, which are uncertain, or wanting in that ſcience, determined with the greateſt exactneſs: In a word, geography, may, by ſuch an expedient, arrive, in a very expeditious manner, to as great a degree of perfection, as it is capable of.

Mr. *Greenwood* finiſhes his deſign, when he has taken notice of the method of determining the degree, and ſtrength

of the wind from ſuch *data* relating thereto, as are to be found in ſea-journals; which is in general from obſerving how many knots the veſſel goes at the time of obſervation, and which is always inſerted in the day-book or journal; or in other words, what velocity ſhe then has; for, the ſtrength of the wind may with exactneſs enough in this affair, be judged of from the effect it produces, or the motion it communicates to the ſhip. It is true, there will be ſome conſiderable difference in this reſpect, ariſing from the ſhape, and burden of the veſſel: However, as we do not expect a mathematical exactneſs in this article, after a little uſe and experience, together with comparing the greateſt velocities of different ſhips together, a perſon may ſeldom fail of judging of the ſtrength of the wind, at leaſt to a fourth part; that is, if according to the method propos'd above, the greateſt winds be expreſs'd by 4, and the ſmalleſt by 1.

In oblique winds, the ſtrength or degree thereof will not be directly proportional to the velocity of the veſſel, but muſt be corrected a little: However, there will be no difficulty in this matter. For, ſuch as are acquainted with the method of reſolving oblique powers into direct ones, may eaſily compoſe a table of proportional parts, correſponding thereto.

Some Obſervations towards compoſing a Natural Hiſtory of Mines *and* Metals; *by Dr.* Nicholls. Phil. Tranſ. N° 401. p. 402.

THE following are the particulars of what Dr. *Nicholls* obſerv'd during a year's ſtay in the weſtern parts of *Cornwall*, concerning mines, *&c.*

Mines in general are veins or cavities within the earth, whoſe ſides receding from, or approaching nearer to each other, make them of unequal breadths in different places; ſometimes forming large ſpaces, which are call'd *holes*: they are fill'd with ſubſtances, which, whether metallic, or of any other nature, are call'd the *loads*: When the ſubſtances forming theſe *loads* are reducible to metal, the loads are by the miners ſaid to be alive; otherwiſe they are call'd dead *loads*.

In *Cornwall* and *Devon* the loads always hold their courſe from eaſtward to weſtward; tho' in other parts of *England* they

they frequently run from north to south. The miners report, that the sides of the *load* never bear in a perpendicular, but constantly underlay, either to the north or south.

The mines seem to be, or to have been, the channels thro' which the waters pass within the earth; and like rivers, have their small branches opening into them in all directions; which are by the miners called the feeders of the *load*.

Most mines have streams of water running thro' them, and when they are found dry, it seems to be owing to the waters having changed their course, as being obliged to it, either because the *load* had stopped up the ancient passages, or that some new, and more easy ones are made.

The *load* is frequently intercepted by the crossing of a vein of earth or stone, or some different metallic substance. In which case it generally happens, that one part of the *load* is moved a considerable distance to one side. This transient *load* is by the miners called a *flooking*; and the part of the *load*, which is moved is, according to them, said to be *heav'd*. This heaving the *load* would be an inexpressible loss to the miner, did not experience teach him, that as the *loads* always run on the sides of the hills, so the part heaved is always moved towards the descent of the hill: So that the miner, working towards the ascent of the hill, and meeting a *flooking*, considers himself as working in the part heaved; wherefore, cutting thro' the *flooking*, he works upon its back towards the ascent of the hill, till he recover the *load*, and *vice versâ*.

AD (Fig. 6. Plate VII.) represents a *load* running in the side of a hill; B the *feeders*; C the *flooking*; D the part *heav'd*.

Sometimes, tho' not constantly, the mine is lined with an intermediate substance between the load and itself: This, properly speaking, is the wall of the load; tho' in the common acceptation of that term, it signifies either such intermediate substance, or the side of the mine, where the load immediately unites itself to it.

A (Fig. 7.) represents the side of the mine; B the intermediate wall of white mundic; C the load of copper.

DE (Fig. 8.) represents two walls of spar-stone; F a small vein of tin ore.

The springs in these parts are always hard, as abounding very much, either in stony, or sulphureo-saline particles.

From this water thus saturated with stony particles, we frequently find the passages of the water under ground, either partly,

partly, or totally stopped up; the stony matter gradually concreting round the sides of the mine, and thereby forming a confused load of spar-stone.

At other times this stony matter concretes more distinctly: In which case the stony matter seems to be governed in its concretion by a plastic power.

N. B. When the Dr. speaks of a plastic power, he would be understood as meaning only a *modus* of attraction, by which the attracted particles are ranged in this or that determinate form. This power then exerts its action in such a manner, as to range the concreting matter into the form of a hexagonal prism, whose *apex* goes off in a hexagonal pyramid. Where this plastic power happens to be single, and uncontrouled, it preserves the form of the crystal to very considerable magnitudes.

In these single crystals we may observe, that they are of different transparencies and colours, as the stony matter is more or less disengaged from other substances, or as those other substances are capable of imparting different tinctures to them; and that they seem formed into *laminæ*, tho' these last are only distinguishable, when the substances from which the crystal is successively formed, happen to differ in purity.

The crystal A (Fig. 9.) was at first formed from matter intangled with a foul yellow substance; after which, a pure matter acceding, the crystal was in its future lamination formed more pure and transparent.

But where the plastic particles are more numerous, there seems reason to believe, that these very plastic particles, before they are fixed, are subject to the controul, and direction of any fixed plastic particle, within the sphere of whose activity they happen to move: Notwithstanding which, after they are once fixed, they exert their own plastic powers, and in conjunction with the first plastic particle, govern the future concretion, in such a manner as to form a seemingly irregular crystal, tho' composed of two or more regular crystals.

Thus A and C (Fig. 10, 11.) seem to have attracted among the stony particles two plastic particles, which afterwards exerting their own powers, form the additional crystals B and D. There are several phenomena observable in these crystals, which the Dr. passes over, as less relating to the affair of metals. Wherefore, he only adds, that these crystalline concretions exert a strong attraction on several metallic substances. As the spar A (Fig. 12.) has attracted the three portions of lead B

an

and the cryſtals C (Fig. 13.) have attracted the copper D, and are attracted by the lead E.

The ſulphureo-ſaline particles, with which, as the Dr. has obſerved, the waters are frequently ſaturated, are found to be either of a vitriolic or arſenical nature: The firſt, if pure, conſtantly concreting into white cubes, reſembling grains of ſilver; while the arſenical ſulphur concretes into yellow cubes like grains of pure gold: Both theſe are by the miners termed mundic.

Theſe ſulphureo-ſaline particles ſeem directed in their concretions by a plaſtic power, in the ſame manner as the cryſtals abovementioned; and like them, upon the ſame principles, are found ſimple or compound. In their ſides you may obſerve the concretion forms itſelf like threads, which in three ſides run in different directions, but are always ſimilar in the oppoſite ſides.

Fig. 14. repreſents one of theſe cubes; A the parallel threads.

Fig. 15. repreſents another of theſe cubes, from whoſe ſides ariſe ſmall ſegments of cubes, as C.

But this plaſtic power ſeems to be weakened, or deſtroyed, in proportion, as this ſulphureous matter is more or leſs intangled with metallic ſubſtances.

Thus in Fig. 16. the plaſtic particle ſeems for a while to have exerted its power in the uſual manner, till the adventitious matter became intangled with a ſmall quantity of copper, after which it ſeems only to have exerted its attractive, but not its plaſtic power.

And in Fig. 17. the white mundic being infected with iron, ſeems ſo far from being affected by a plaſtic power, that it concreted in the form of icicles from the fluid which tranſuded thro' the top of the mine.

Fig. 18. repreſents ſome ſmall cubes of white, or vitriolic mundic.

But to return to the mines; They are found to contain iron, tin, lead, copper, and a pſeudo-metallic ſubſtance, by the miners termed gliſt.

Eclipſes

Eclipses *of the first Satellite of* Jupiter *at* Lisbon *in* 1726; *by* F. Carbone. Phil. Transf. N° 401. p. 408. *Translated from the* Latin.

		Emersions	True time h.	′	″	
1725						
Sept.	23.	Emersion at	6	56	42	
Oct.	16.	Emersion	7	15	17	
Nov.	8.	Emersion	7	31	33	
	15.	Emersion	9	26	52	
Dec.	17.	Emersion	5	56	34	
	24.	Emersion	7	49	18	Doubtful
1726						
Jan.	9.	Emersion	6	2	3	Doubtful

		Immersions	True time.			
1726						
May	23.	Weakening of the light at	15	24	0	
		Total immersion	15	24	40	
July	1.	Weakening of the light	13	46	29	
		Total immersion	13	47	47	
	8.	Weakening of the light	15	40	30	
		Total immersion	15	41	40	
	17.	Weakening of the light	12	0	45	
		Total immersion	12	1	52	
Aug.	9.	Total immersion	12	13	30	
	16.	Impairing of the light	14	7	33	
		Total obscuration	18	8	46	
	23.	Impairing of the light	16	3	10	
		Total immersion	16	4	23	
	25.	Weakening of the light	10	31	40	
		Total obscuration	10	32	57	
Sept.	1.	Impairing of the light	12	27	57	
		Total immersion	12	29	29	
	10.	Weakening of the light	8	53	47	
		Total obscuration	8	54	54	
	15.	Immersion into the shadow	16	21	32	
	17.	Impairing of the light	10	50	12	
		Total immersion	10	51	39	
	24.	Weakening of the light.	12	46	38	
		Total obscuration	12	47	45	Doubtful
Oct.	10.	Total immersion	11	8	34	somewhat doubtful Immer

1726	Immersions	True time h.	′	″	
Oct. 26.	The beginning of the emersion	11	39	41	
	The light entirely restored	11	41	2	
28.	Emersion out of the shadow	6	8	54	
	Emersion out of the *penumbra*	6	10	4	
Nov. 4.	Beginning of the emersion at	8	6	3	
	Total emersion	8	7	14	
11.	Beginning of the emersion	9	57	39	
	The light entirely restored	9	58	49	
20.	Emersion out of the shadow.	0	20	19	
	Emersion out of the *penumbra.*	5	21	29	
Dec. 4.	Emersion out of the shadow	10	5	18	Doubtful
	Total recovery of the light	10	6	34	

Observations on Dissecting the Body *of a Person troubled with the* Stone; *by Mr.* John Ranby. Phil. Trans. N° 401. p. 413.

A Man 70 years of age died of a suppression of urine, occasioned by a stone of the bigness of a common-bean stopping in the *urethra*, just within the *glans*. This appearance, with the symptoms that had attended this miserable patient, gave Mr. *Ranby* reason to expect something remarkable in the urinary passages. The *ureters* and *pelvis* were exceedingly distended; which is common where great numbers of stones have descended down thro' them from the kidneys to the bladder. The bladder contained about 60 stones, the largest of which was about the size of a walnut, the others smaller; and just within the neck of the bladder was a hard tumour, as big as a nutmeg, which almost closed the orifice; and indeed, the situation of this tumour was such, that it not only made the passing the *catheter* very difficult, and hindered his feeling the stones, by directing the instrument upwards; but likewise would alone produce the symptoms of the stone in the bladder, by obstructing the free discharge of urine thro' the *urethra*; the inner membrane of which appeared, as if lacerated in several places, and the tube filled with a glutinous matter, tinged with blood. On the back part of the *vesiculæ seminales*, near the *prostatæ*, were several stones, as big as grains of pease, which closely adhered to the adjacent membranes.

Obſervations of the Eclipſes *of* Jupiter's Satellites *from the Year* 1700 *to* 1727; *by Mr.* Derham. Phil. Tranſ. N° 402. p. 415. *Tranſlated from the* Latin.

Eclipſes of Jupiter's firſt Satellite.

I. Satel. Days of the month		Equal time. H.	M.	S.	Apparent time. H.	M.	S.	By Flamſtead's and Caſſini's Tables. H.	M.		Quality of the eclipſe.	Heliocentric place of Jupiter.	Degrees	
					Anno 1700.									
Aug.	13.	10	59	4	10	57	10		59	Flam.	Emerſion	Aquar.	15.	with a 6 foot teleſ.
Dec.	1.	—	—	—	5	1	8	4	55½	Caſ.	Emerſion	Aquar.	10.	with a 16 foot teleſ.
					—	1	38	4	58	Flam.				

All the following eclipſes were obſerved with a 16 foot teleſcope, unleſs it be otherwiſe ſignified.

					1701									
June	15.	—	—	—	13	23	0	13	21	Flam.	Immerſ.	Aquar.	27.	
					—	24	50	13	26	Caſs.				
July	8.	—	—	—	13	30	0	13	28	Flam.	Immerſ.	Aquar.	29.	
								13	34	Caſs.				
					5	54	9							
Oct.	12.	—	—	—	—	54	19	5	59	Caſs.	Emerſ.	Piſc.	8.	good obſervation
					—	54	49							
	19	—	—	—	7	48	57	7	55	Caſs.	Emerſ.	Piſc.	9.	good obſervation
					—	49	47							
Dec.	20.	—	—	—	6	25	0	6	28	Flam.	Emerſ.	Piſc.	14.	doubtful

Eclipses of Jupiter's first Satellite.

I. Satel. Days of the month		Equal time. H.	M.	S.	Apparent time. H.	M.	S.	By Flamstead's and Cassini's Tables. H.	M.		Quality of the eclipse.	Heliocentric place of Jupiter.	Degrees	
					Anno 1702									
					9	22	0	9	23	Flam.				
Oct.	15.	—	—	—	—	22	15	9	26	Cass.	Emersion	Aries	11 ½	very good.
					—	22	45							
					5	44	57	5	47	Flam.				
	24	—	—	—	—	45	21	5	50	Cass.	Emersion	Aries	12	very good.
					—	45	42							
Dec.	9.	—	—	—	5	50	0	6	5	Flam.	Emersion	Aries	16 ½	air foggy.
					1703									
Aug.	8.	—	—	—	15	21	30	15	18	Flam.	Immers.	Taur.	8. ½	doubtful by reason of fogs.
	24.	—	—	—	13	43	6	13	38	Flam.	Immers.	Taur.	10.	good
						43	35	13	45	Cass.				
Sep.	2.	—	—	—	10	8	20	10	4	Flam.	Immers.	Taur.	11.	Sky overcast
Nov.	28.	5	37	40		44	52	5	43	Flam.	Emers.	Taur.	19.	pretty good

Eclipses of Jupiter's first Satellite.

I. Satel. Days of the month		Equal time.			Apparent time.			By Flamstead's and Cassini's Tables.			Quality of the eclipse.	Jupiter's place.		
		H.	M.	S.	H.	M.	S.	H.	M.			Degrees		
Anno 1704														
Aug.	26.	17	7	43	17	9	53	17	8	Flam.	Immers.	Gem.	12 ¼	too much light but pretty good
	28.	11	36	16	11	38	18	11	37	Flam.	Immers.	Gem.	13	good
					—	39	3							
Sep.	4.	13	29	14	13	33	5	13	23	Flam.	Immers.	Gem.	13 ½	good
					—	34	25							
Oct.	6.	10	2	40	10	16	40	10	13	Flam.	Immers.	Gem.	16	foggy but pretty good
					—	17	10							
Nov.	3.	17	39	54	17	54	55	17	48	Flam.	Immers.	Gem.	18 ¼	Sky overcast
Dec.	9.				5	31	00				Emers.	Gem.	21 ¾	doubtful
	23.	9	14	43	9	9	17	9	10	Cass.	Emers.	Gem.	23	good

Eclipses of Jupiter's first Satellite.

I. Satel. Days of the month		Equal time. H.	M.	S.	Apparent time. H.	M.	S.	By Flamstead's and Cassini's Tables. H.	M.		Quality of the eclipse.	Jupiter's Place. Degrees	
Anno 1705													
					9	46	3						
Mar.	2.	—	—	—	—	46	40	9	47	Flam.	Emersion	Gem. 29.	good
					—	47	0						
		10	9	58	10	7	18						
	25.				—	7	52	10	10	Flam.	Emersion	Cancer 0 ½	good
		10	11	5	—	8	25						
		16	39	15	16	45	26						
Sept.	7.	—	40	15	—	46	26	16	52	Flam.	Immers.	Cancer 15	good
Oct.	30.	18	54	50	19	10	19	19	10	Cass.	Immers.	Cancer 19	too much light therefore doubtful
			55	20	—	10	49						
Nov.	22.	19	4	50	19	13	59				Immers.	Cancer 21	

Eclipses of Jupiter's first Satellite.												
I. Sat. Day of the month.	Equal time			Apparent time.			By Flamstead's and Cassini's tables.			Quality of the eclipse.	Place of Jupit.	
	H.	M.	S.	H.	M.	S.	H.	Min:			Degrees.	
				Anno 1705.								
Dec. 15.	19	15	0	19	13	20	19	11	Flam.	Immersion	Cancer 22 3:4	At Canterbury Mr. Gray obser. this immmers. at 19h. 15′.
				1706								
March 7.	—	—	—	7	27	0	7	26	Flam.	Emersion.	Cancer 29 1:2	Mr. Derham found it emerg.
30	7 —	46 47	40 37	7 —	45 46	25 20	7	48	Flam.	Emersion.	Leo 3	Good.
April 29.	9	59	2	10	3	5	10	3	Flam.	Emersion.	Leo 3 3:4	Sky overcast.

Eclipses of Jupiter's first Satellite.

I. Sat. Day of the month.		Equal time.			Apparent time.			By Flamstead's and Cassini's Tables.			Quality of the eclipse.	Place of Jupit.			
		H.	M.	S.	H.	M.	S.	H.	Min.				Degrees.		
					Anno 1707.										
Feb.	15.	10	17	52	10	4	43	10	3	Flam.	Emersion	Leo	26	3:4	
	24	6	39	52	6	28	20	6	28	Flam.	Emersion	Leo	27	1:4	Good
		—	41	32	—	30	0								
March	26.	8	47	53	8	45	36	8	46	Flam.	Emersion	Leo	29	3:4	A strong wind shook the telef.
		—	48	46	—	45	49								
May	11.	9	14	43	9	48	43	9	20	Flam.	Emersion	Virgo	3	1:4	Good
		—	16	7	—	20	7								
					1708										
Jan.	31				17	35	0				Immersion	Virgo	23	1:2	Immerg. before.
					1709										
May	18.	9	7	26	9	10	47	9	18	Flam.	Emersion.	Libra	1	3:4	Pretty good.
		—	8	26	—	11	47	8	59	Cass.					
June	10.	9	22	21	9	21	9	9	27	Flam.	Emersion.	Libra	3	1:2	Good.
		—	24	0	—	23	8	9	12	Cass.					

Eclipses of Jupiter's first Satellite.

I. Sat. Day of the month.	Equal time.			Apparent time.			By Flamst. and Cassini's tables.			Quality of the eclipse.	Place of Jupiter		
	H.	M.	S.	H.	M.	S.	H.	Min.				Degrees.	
1710													
May 14.	10	6	57	10	10	26	10	14	Flam.	Emersion.	Scorpio	26	
July 15.	—	—	—	8	46	0	8	49	Flam.	Emersion.	Sagitt.	2	Mr, Derham obs. immerged.
August 23	7	22	2	7	22	50	7	28	Cass.	Emersion.	Sagitt.	5	Pretty good.
1711													
Aug. 19.	—	—	—	8	23	40	8	32	Flam.	Emersion.	Capri.	4	Good.
				—	24	30	8	30	Cass.				
				—	25	0							
1713													
Oct. 27.	8	10	28	8	26	19	8	35	Flam.	Emersion.	Pisces	14	Good.
Dec. 28.	7	4	18	6	56	30	7	5	Flam.	Emersion.	Pisces	19 3:4	Good.
							7	1	Cass.				

Eclipses of Jupiter's first Satellite.

Oct.	27.	8	10	28	8	20	19	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Dec.	28.	7	4	18	6	56	30	7	5	Flam.	Emersion.	Pisces	19 3:4	Good.
								7	1	Caſs.				

Eclipſes of Jupiter's firſt Satellite

Eclipses of Jupiter's first Satellite.

I. Sat. Day of the month.	Equal time.			Apparent time.			By Flamstead's and Cassini's tables.			Quality of the eclipse.	Place of Jupit.		
	H.	M.	S.	H.	M.	S.	H.	M.				Degrees.	
				1714									
Oct. 23.	9	59	0	10	1	1	10	5	Flamst	Emersion	Aries	17	Doubtful.
Nov. 1.	—	—	—	6	20	0	6	20	Flamst	Emersion	Aies	18	The sky overcast, therefore doubtful, tho' Mr. Derham took the observation with a 34 foot telescope.
				1717									
	6	25	3	6	10	15							
Feb. 1.	—	26	18	—	11	30				Emersion.	Cancer	1	Good.
	—	26	48	—	12	00	6	14	Flamst				
	8	21	21	8	7	0	6	14	Cass.				
8	—	21	51	—	7	30	8	9	Flamst	Emersion.	Cancer	1 1:2	Good.
	—	22	21	—	8	0	8	12	Cass.				

Eclipses of Jupiter's first Satellite.

The following observations were made with a very good 12 and $\frac{1}{2}$ foot telescope.

I. Sat. Day of the month.		Equal time.			Apparent time.			By Mr. Bradley's tables.			Quality of the eclipse.	Place of Jupit.		
		H.	M.	S.	H.	M.	S.	H.	M.			Degrees.		
1725														
Nov.	27.	—	—	—	9	8	36	9	6	Brad.		Pisces	22	At Windsor; doubtful.
					—	9	0							
1726														
		7	40	19	7	29	49							
Jan.	5.	—	41	0	—	30	0	7	31	Brad.	Emersion	Pisces	25	At Upminster, good.
		—	41	30	—	31	0							
Aug.	5.	14	50	17	14	46	28	14	47	Brad.	Immers.	Aries	15	Good.
		—	51	7	—	47	18							
		7	44	17		53	10							
Sep.	15.	—	45	37	7	54	30	7	53	Brad.	Immers.	Aries	19	Good.
		—	46	7	—	55	0							
Oct.	8.	—	—	—	10	25	0	10	7	Brad.	Emersion	Aries	21	This observation doubtful by reason of Jupiter's nearness.

Eclipses of Jupiter's first Satellite

I. Sat. Day of the month.	Equal time. H.	M.	S.	Apparent time H.	M.	S.	By Mr. Bradley's tables. H.	M.		Quality of the eclipse.	Place of Jupit.	Degrees	
							1726						
Oct 17.	—	—	—	6	46	30	6	46	Brad.	Emerf.	Aries	21 1:2	
Dec. 2.	—	—	—	7	2	0	7	5	Brad.	Emerf.	Aries	25	At Windsor; doubtful.
				—	3	0							
25	7	12	57	7	7	0	7	9	Brad.	Emerf.	Aries	28	At Upminster, good.
	—	13	57	—	8	0							

Eclipses of Jupiter's second Satellite.

The following observations were made with a 16 foot telescope.

II. Sat. Day of the month.	Equal time. H.	M.	S.	Apparent time. H.	M.	S.	By Flamstead's tables. H.	M.	Quality of the eclipse.	Place of Jupit.	Degrees.	
				Anno 1700								
Oct. 27	—	—	—	8	24	0	8	23	Emersion.	Aquar.	6 3:4	Doubtful, by reason of vapou. observ'd by Mr. Flamstead's servant about 8h. 16' p.m.

Eclipses of Jupiter's second Satellite.

II. Sat. Day of the month.	Equal time	Apparent time.	By Flamstead's and Cassini's tables.	Quality of the eclipse.	Place of Jupit.	
	H. M. S.	H.M. S.	H. Min:		Degrees.	
		Anno 1701.				
June 29	— — —	10 50 0	10 52	Im merf.	Aquar 28 1:2	Doubtful, by reason of vapou.
July 31.	Between	9 43 0 10 3 0	10 33	Immerf.	Pisces 1	Sky overcast.
Oct. 21.	— — —	7 39 35 — 40 0 — 41 0	7 51	Emerf.	Pisces 8 3:4	Good.
Oct. 28.	— — —	10 18 2 — 20 0	10 29	Emerf.	Pisces 9 1:2	Good.
Nov. 22.	— — —	7 26 18 — 27 0	7 34	Emerf.	Pisces 11 1:2	Good.

Eclipses of Jupiter's second Satellite.

II. Sat. Day of the month.		Equal time.			Apparent time.			By Flamstead's and Cassini's Tables		Quality of the eclipse.	Place of Jupit.		
		H.	M.	S.	H.	M.	S.	H.	Min.		Degrees.		
Anno 1702.													
Aug.	26.	—	—	—	9	46	0	10	17	Immerſ.			Mr. Derham found it immerged.
Sep.	9.	—	—	—	15	0	51	15	35	Immerſ.	Aries	8	Good.
					—	1	51						
					7	5	21						
Oct.	15.	—	—	—	—	6	22	7	30	Emerſion	Aries	10 1:2	Good.
					—	7	22						
					9	40	38						
	22.	—	—	—	—	41	0	18	8	Immerſ.	Aries	12	Good.
					—	42	13						
1703													
Aug.	20.	—	—	—	9	50	0	10	11	Immerſ.	Taur.	9 3:4	M. Derham found it immerged.
Oct.	5.	—	—	—	15	3	0	15	19	Immerſ.	Taur.	24	Immerg. before
Dec.	19.	8	41	9	8	38	7	9	6	Emerſion.	Taur.	20 1:2	Good.

Eclipses of Jupiter's second Satellite.											
II. Sat. Day of the month.	Equal time.			Apparent time.			By Flamst. and Cassini's tables.		Quality of the eclipse.	Place of Jupiter	
	H.	M.	S.	H.	M.	S.	H.	Min.		Degrees.	
							1704				
August 20	12	15	29	12	15	34	12	32	Immers.	Gemini 12	Good.
Oct. 5.	—	—	—	17	10	44	17	32	Immers.	Gemini 16	Good.
				—	11	14					
16.	—	—	—	9	2	19	9	32	Immers.	Gemini 10	Pretty good.
							1705				
Jan. 12	—	—	—	7	47	0	8	14	Emersion.	Gemini 24 3:4	Emerged before
20	10	30	23	10	16	9	10	50	Emersion.	Gemini 25 1:4	Good.
	—	31	14	—	17	0					
Feb. 14	7	45	31	7	32	0	8	1	Emersion.	Gemini 27 1:2	The air full of vapours; doubtful.
March 25	10	15	10	10	12	30	10	36	Emersion.	Cancer 0	Uncertain by rea. of the nearness of the 1 sat
	—	16	5	—	13	25					
April 26	—	—	—	10	4	0	10	29	Emersion.	Cancer 3 3:4	The horizon full of vapours, doubtful.

Eclipses of Jupiter's second Satellite.

II. Satel. Days of the month		Equal time. H.	M.	S.	Apparent time. H.	M.	S.	By Flamstead's and Cassini's Tables. H.	M.	Quality of the eclipse.	Jupiter's Place.	Degrees	
					Anno 1705								
Sept.	29.	16	26	11	16	39	11	16	48	Immers.	Cancer	16 ½	good
		—	26	26	—	39	26						
Oct.	31	15	55	20	16	10	52	16	27	Immers.	Cancer	19	good
		—	56	26	—	11	49						
Dec.	20.	9	49	30	9	45	36	10	11	Immers.	Cancer	23	good
		—	50	10	—	46	16						
					1706								
Apr.	20.	—	—	—	8	58	30	9	31	Emersion	Leo	3	emerged
					1707								
Mar.	13.	—	—	—	7	59	0	8	18	Emersion	Leo	28 ¼	emerged before
	20.	10	28	14	10	23	49	11	5	Emersion	Leo	29	doubtful
Apr.	14.	7	33	17	7	35	39	8	9	Emersion	Virgo	0 ½	doubtful by reason of too much light
	21.	10	9	33	10	12	51	10	45	Emersion	Virgo	1 ½	good
		—	10	59	—	14	17						

Eclipses of Jupiter's second Satellite.						
II. Satel. Days of the month	Equal time. H. M. S.	Apparent time. H. M. S.	By Flamstead's and Cassini's Tables. H. M.	Quality of the eclipse.	Heliocentric place of Jupiter. Degrees	
		Anno 1710.				
Mar. 4.	— — —	17 5 0		Immerf.	Virgo 21 $\frac{3}{4}$	cloudy and doubtful
		1711				
July 15.	— — —	9 16 0	9 6	Emerf.	Capric. 1 $\frac{1}{2}$	foggy and doubtful
		1712				
Oct. 12.	7 35 6 — — —	7 50 30 — 51 15 — 52 30	7 58	Emerf.	Aqua. 10	good
		1714				
Nov. 1.	— — —	6 34 7	6 55	Emerf.	Aries 18	doubtful by reason of clouds.
Dec. 3.	— — —	6 14 26 — 15 0	6 34	Emerf.	Aries 21 $\frac{1}{2}$	good
		1716				
Dec. 22.	— — —	4 55 0	— — —	Emerf.	Gem. 27 $\frac{1}{2}$	emerged before.

Eclipses of Jupiter's second Satellite.

II. Satel. Days of the month	Equal time. H. M. S.	Apparent time. H. M. S.	By Flamstead's and Molyn's calculation. H. M.	Quality of the eclipse.	Jupiter's place. Degrees	
		Anno 1717				
Jan. 30	— — —	7 24 0	7 46	Emerf.	Cancer 1	Mr. Derham found it emerged.
		1725				
Oct. 30.	— — —	9 16 23 — 17 0 — 17 18	8 46 Mol.	Emerf.	Pifc. 19.	good obfervation with a very good 12 and ½ foot tel.
		1726				
Aug. 18.	8 52 25 — 53 55	8 54 52 — 56 22	8 58 Mol.	Immerf.	Aries 17	good
Oct. 17.	— — —	6 12 0	5 53 Mol.	Emerf.	Aries 21 ½	doubtful
Nov. 25.	— — —	8 27 15 — 28 0	8 16.	Emerf.	Aries 15	pretty good at Windfor

Eclipses of Jupiter's third Satellite.

III. Satel. Days of the month	Equal time.			Apparent time.			By Flamstead's and Molyn's calculation.		Quality of the eclipse.	Heliocentric place of Jupiter.	
	H.	M.	S.	H.	M.	S.	H.	M.		Degrees	
				Anno 1700							
Oct. 19.	—	—	—	6	23	15	6	1	Immerſ.	Aquar. 6.	
				1701							
July 11.	—	—	—	10	18	0	10	11	Immerſ.	— — —	doubtful
Sep. 28.	—	—	—	10 — —	25 26 26	27 0 42	10	12	Emerſ.	Piſc. 6 ½	good
				1702							
July 26	—	—	—	10	51	13	10	33	Emerſ.	Aries 4	doubtful
Aug. 2.	Between			11 12	55 5	0 0	11	35	Immerſ.	Aries 5	clouds interrupted

III. Sat. Day of the month.	Equal time.			Apparent time.			Flamſtead's calculation.		Quality of the eclipſe.	Place of Jupit.	
	H.	M.	S.	H.	M.	S.	H.	M.		Degre es.	
							1703				
July 12.	—	—	—	14	46	0	15	32	Immerſ.	Taurus 6	immerged
Aug. 24.	—	—	—	13	43	20	13	4	Immerſ.	Taur. 10.	A very good obſervation of the immerſion; but by an accident he was too late to ſee the emerſion.
				—	44	20					
				—	44	51					
				16	6	36	15	40	Emerſ.		
Sept. 22.	—	—	—	8	9	53	7	50	Emerſ.	Taur. 12¾	
29.				9	0	44	9	17	Immerſ.	Taur. 13	The immerſion was accurately obſerv'd, but the emerſion was doubtful by reaſon of *Jup.* nearneſs.
	—	—	—	—	1	5	11	52	Emerſ.		
				12	2	17					
Oct. 6.	13	46	13	14	0	30	13	19	Immerſ.	Taur. 14	Good.
	—	47	33	—	1	50					
	—	48	33	—	2	50					

In the obſervations of *Aug*. 24. *Sept*. 22, 29 and *Oct*. 6. it is to be noted that the latitude of the third ſatellite was greater than either Mr. *Flamſtead* or M. *Caſſini* conjectur'd; for it was elongated as far as the extreme edge of *Jupiter*'s pole; and (before it was entirely immerged into his ſhadow) continu'd hid for ſome time in his penumbra; and Mr. *Derham* thinks it was not in the ſhadow above two hours, tho' it ſeem'd to have a longer *mora* after the obſervations *Aug*. 24. and *Sept*. 29: But in the former of theſe he did not ſee the true emerſion; and in the latter, the emerſion was ſo near *Jup*. limb, that it was hard to obſerve it truly with a 16 foot teleſcope. Sun and *Jup*. in *Sext*. aſp

Eclipſes of Jupiter's third Satellite.

III. Sat. Day of the month.	Equal time.			Apparent time.			Flamſtead's calculation.		Quality of the eclipſe.	Place of Jupit.	
	H.	M.	S.	H.	M.	S.	H.	M.		Degrees.	
				Anno 1794							
Sept. 14.	Between		—	16 17	18 10	0 0	16	32	Emerſ.	Gem. 14 1/4	Sky overcaſt
Oct. 20.	10	14	34	10 —	28 30	40 33	9	48	Immerſ.	Gem. 17	Good
Nov. 3.	18	12	16	18	27	17	17	45	Immerſ	Gem. 18 3/4	Good, tho' the sky was overcaſt
				1705							
Jan. 14	10 —	7 9	5 4	9 —	53 55	57 56	—	—	Immerſ.	Gem. 24 1/4	Good.
	—	—	—	9	58	7					
Feb. 26	10	9	32	—	58	42	—	—	Immerſ.	Gem. 28 1/2	Good.
Oct. 6	Between			17 —	18 27	0 0	—	—	Emerſion	Cancer 17	Doubtful, by reaſon of thick foggs.
Nov. 25	—	—	—	7	41	0	—	—	Immerſ.	Cancer 21	

Eclipses of Jupiter's third Satellite.

III. Sat. Day of the month.	Equal time H.	M.	S.	Apparent time. H.	M.	S.	By Flamstead's calculation H.	Min:	Quality of the eclipse.	Place of Jupit.	Degrees.	
							1706					
Mar. 13	—	—	—	8	52	27	7	36	Emersion.	Leo	0	Good.
				—	54	0						
20	—	—	—	9	24	0	9	3	Immers.	Leo	0 1 :2	Doubtful, by reason of Jup. nearness.
Nov. 4.	—	—	—	16	59	27	16	50	Immers.	Leo	0 3:4	pretty good.
							1707					
May 17	—	—	—	8	17	0	—	—	Emersion	Virgo	3 1:4	Mr Derh. found it emerged.
Nov. 26.	18	41	37	18	49	15	18	41	Emersion	Virgo	18 1:2	
Dec. 3	19	19	38	19	24	1	19	25	Immers.	Virgo	19	Good.
	—	20	46	—	25	9						
							1709					
Feb. 5.	17	35	32	17	21	0	17	26	Immers.	Libra	21 1:2	
	—	37	4	—	22	0						

Eclipses of Jupiter's third Satellite.

III. Sat. Day of the month.	Equal time. H.	M.	S.	Apparent time. H.	M.	S.	By Flamstead's calculation H.	Min.	Quality of the eclipse.	Place of Jupit. Degrees.	
						1711					
July 14.	—	—	—	9	45	0	9	41	Emersion	Aquarius 2	doubtful.
						1712					
				6	15	0	6	28	Immers.		Doubtful by reason of too much light.
				—	20	0					
Sep. 16.	—	—	—	9	50	3				Aquar. 7 3:4	Good.
				—	52	0	10	5	Emersion		
				—	52	30					
Oct. 29.				6	26	30	6	45	Immers.	Aquar. 11 1:2	Good.
				—	28	0					
						1713					
Dec. 26.	6	38	30	6	32	0	6	46	Emers.	Pisces 19 1:2	The wind render'd it doubtful.
						1714					
Sep. 17	8	26	56	8	36	9	8	40	Immers.	Aries 13 3:4	Good, with a 34 foot telescope.
	—	28	56	—	38	9					
	7	22	0	7	24	0					
Oct. 23.	—	23	0	—	25	0	7	38	Emers.	Aries 17	Good, with a 16 foot telescope.
	9	25	0	—	27	0					

Eclipses of Jupiter's third Satellite.

III. Satel. Days of the month	Equal time. H.	M.	S.	Apparent time. H.	M.	S.	By Flamstead's calculation. H.	M.	Quality of the eclipse.	Jupiter's Place. Degrees	
				Anno 1716							
Dec. 5.	—	—	—	—	—	—	7	37	Emersion	— — —	This satellite has a considerable latitude, and if Mr. Derham mistake not, it was not eclipsed this night.
				1717							
Jan. 17.	—	—	—	5	52	23	—	—	Immerf.		good
				—	53	45	—			Cancer 0	
				8	48	38			Emersion		bad
				1726							
Jan. 5.	6	41	10	6	30	40	—	—	Emersion	Pisces 25 $\frac{1}{4}$	good with a 12 and $\frac{1}{2}$ foot telescope
	—	42	30	—	32	0					
Dec. 15.	7	13	29	7	12	17	7	21 Mol.	Emersion	Aries 15 $\frac{1}{4}$	good
	—	14	42	—	13	30					

Eclipses of Jupiter's fourth Satellite.

IV. Satel. Days of the month	Equal time. H.	M.	S.	Apparent time. H.	M.	S.	By Flamstead's calculation. H.	M.	Quality of the eclipse.	Heliocentric place of Jupiter.	Degrees	
				Anno 1701.								
June 11.	—	—	—	14	20	0	14	35	Immerf.	Aqua.	27	Mr. Derham found it immerged.
Sep 3.	—	—	—	13	59	0	14	21	Emerf.	Pifc.	4 1/4	thro' the fog he found it immer.
				1704								
Sep. 30.	—	—	—	9	21	42	9	40	Emerf.	Gem.	15 3/4	Sky overcaft, therefore doubtful
Dec. 6.	—	—	—	9	52	44	—	—	Emerf.	Gem.	21 1/2	doubtful by reafon of Jupiter's nearnefs and the Satellite's fmallnefs
				1705								
Feb. 11.	8	38	5	8	24	8	8	23	Immerf.	Gem.	27	pretty good
				1706								
Mar. 20.	9	3	2	8	58	40	9	36	Immerf.	Leo	0 1/4	pretty good
Sep. 20.	—	—	—	16	24	0	16	17	Immerf.	Leo	15	Mr. Derham found it immerged
				1712								
Aug. 20.	between			8 —	29 39	0 0	10	11	Immerf.	Aquar.	5.	
Sept. 6.	—	—	—	7 — —	43 44 45	0 0 0	9	20	Emerf.	Aquar.	7.	good

Remarks on the foregoing tables.

As exact tables to calculate the eclipses of *Jupiter*'s satellites would be of very great service to find the longitude of places: So Mr. *Derham* has some hopes that these observations of some of them in more revolutions than one of *Jupiter* in his orbit, may be of use to correct, or make such tables.

The greatest chasms in them were occasioned by some dangerous fits of sickness, which impaired Mr. *Derham* in such a manner, that ever after he dared not to venture on making observations at unseasonable hours of the night.

As to his manner of observing, it was for the most part with a 16 foot telescope, and afterwards with an excellent one not inferior to it, of 12 foot and $\frac{1}{2}$, which, at *Jupiter*'s light, bears an aperture of two inches and a half, and a charge of about two inches.

And as to the time, Mr. *Derham* made use of an excellent and well adjusted clock, corrected at noon, by the meridional transits of the sun, observed with the instrument described in *Phil. Trans.* N° 291, which shews the time at noon to 1 or 2 seconds. This way some of Mr. *Derham*'s skillful friends suspected to be fallacious, and not comparable to taking the time by altitudes of the sun, or fixed stars. For a trial, therefore, we observed some eclipses that were agreed on, which, when compared, we found tally so nicely, as to shew to a second of time, or very nearly so, the difference of the meridian of the Observatory at *Greenwich*, and that of *Upminster*.

The greatest part of the observations on the eclipses, that were the most accurately made, may easily be distinguished by the 2, or more numbers of the time of observation: The first of which shews the moment of the beginning of the eclipse; the following, the times when farther advanced: As in an emersion the first number shews the time, when the satellite appears like a small obscure spot; the following numbers, when brighter, or quite emerged out of *Jupiter*'s shadow; and so contrariwise in an immersion.

For greater certainty and satisfaction, Mr. *Derham* has noted which observations were good; which doubtful, or bad: Even the latter of which may be of use in some cases, where better are wanting.

The calculated times of the eclipses he has inserted, where he had them from others, or could calculate them himself; as being of good use to correct the tables of Mr. *Flamstead*, *Cassini*, or others, taken notice of on purpose in the column:

And for the same reason he thought proper likewise to add the place of *Jupiter*.

And lastly, he mentions the length and power of the telescope he made use of; because observations may differ several seconds, by the different length, and goodness of the telescope used: A long and good telescope shewing the satellite, when *Jupiter*'s shadow does but just touch it: Whereas a short, or bad one doth not shew it, till one half, or more of the satellite is illuminated. Which difference is most remarkable in the eclipses of the two outermost satellites, in their greatest latitudes, at which times they immerge into and emerge out of *Jupiter*'s shadow, in an oblique and longer, not a direct and shorter path, an instance may be seen in the observations of the eclipses of the third satellite in *Aug*. and *Sept*. 1703.

Observations on the Heavens *in* 1711 *and* 1712 *in the* Royal Observatory *at* Greenwich; *by Mr*. Flamstead. *Translated from the* Latin.

Time by the clock. H. M. S.	correct time. H. M. S.	Observations of Saturn *Friday Jan*. 5. 1711		Dist. from the vertex
10 14 26	10 14 41	μ *Castor*'s heel Gem. passes		28 50 10
11 11 31	11 11 53	δ in the groin of *Pollux* passes		28 59 0
12 4 45	12 5 0	*Saturn* passes		30 23 20
		R. Ascens. of *Saturn*	119. 1.00.	
		Dist. from the N. pole	68.55.20.	
		Sunday Jan. 7.		
11 2 53	11 0 31	δ of *Gemini* passes		28 59 20
11 50 36	11 48 14	μ in the N. foot of *Cancer* passes		29 5 20
11 55 22	11 53 00	*Saturn* passes		30 21 50
12 15 41	12 13 19	*Bayers* η in *Cancer* passes		30 4 30
12 26 18	12 23 56	γ of *Cancer* the N. *Asellus* passes		28 59 50
		Saturns R. Ascens.	118.52.00	
		Dist. from the N. pole	68.53.50	
		Monday Jan. 8.		
10 58 35	10 57 49	δ of *Gemini* passes		28 59 15
11 50 46	11 50 0	*Saturn* passes		30 20 35
12 11 25	12 10 30	η of *Cancer* passes		30 4 25
		Saturn's R. Ascens.	118.46.30	
		Dist. from the N. pole	68.52.35	

Time

Time by the clock. H. M. S.			Correct time. H. M. S.			Observations of *Saturn*		Dist. from the vertex		
						Thursday Jan. 25. 1711				
9	45	53	9	46	26	δ of *Gemini* passes		28	59	10
10	32	27	10	33	0	*Saturn* passes		30	4	0
11	10	54	11	11	27	δ of *Cancer* S. *Asellus* passes		32	17	50
						Saturn's R. Ascens.	117.23.0			
						Dist. from the N. pole	68.36.0			
						Saturday Jan. 27.				
9	37	25	9	38	2	δ of *Gemini* passes		28	59	20
10	13	11	10	13	48	*l* under the side of *Pollux* passes		30	51	50
10	23	23	10	24	0	*Saturn* passes		30	2	30
10	40	59	10	41	36	*d* behind the tail of *Cancer* passes		32	14	10
10	50	16	10	50	53	*n* of *Cancer* passes		30	4	35
						Saturn's R. Ascens.	117.14 0			
						Dist. from the N. pole	68.34 30			
						Tuesday Jan. 30.				
9	29	45	9	26	55	δ of *Gemini* passes		28	59	40
10	14	50	10	12	0	*Saturn* passes		30	0	0
10	33	20	10	30	30	*d* of *Cancer* passes		32	14	10
10	42	35	10	39	45	*n* of *Cancer* passes		30	4	45
						Saturn's R. Ascens.	117. 0 30			
						Dist. from the N. pole	68.32. 0			
						Wednesday Feb. 28.				
8	18	32	8	15	0	*Saturn* passes		29	43	0
8	27	11	8	23	39	μ in the N. foot of *Cancer* passes		29	5	40
9	2	50	8	59	18	The northern *Asellus* γ of *Can.* passes		29	0	20
10	39	59	10	36	27	The bright star γ in *Leo's* neck passes		30	12	5
						Saturn's R. Ascens.	115.31.30			
						Dist. from the N. pole	68.15. 0			
						Thursday Mar. 1.				
8	14	5	8	11	0	*Saturn* passes		29	42	50
8	22	47	8	19	42	μ of *Cancer* passes		29	5	35
8	58	28	8	55	23	The N. *Asellus* passes		29	0	15
10	35	36	10	32	31	*Lucida Colli Leonis* passes		30	12	0
						Saturn's R. Ascens.	115.30.30			
						Dist. from the N. pole	68.14.50			
						Friday Nov. 9.				
17	21	12	17	21	23	The following to π of *Cancer* passes		35	21	10
17	29	49	17	30	0	*Saturn* passes		34	9	50
18	13	25	18	13	36	The south star *n* in *Leo's* neck passes		33	18	50
						Saturn's R. Ascens.	136.58.0			
						Dist. from the N. pole	72.42.0			

Time by the clock. H.	M.	S.	Correct time. H.	M.	S.	Observations of *Saturn*,	Dist. from the vertex.		
						Monday Nov. 19. 1711.			
16	46	20	16	41	0	*Saturn* passes	34	8	0
16	59	31	16	54	11	The telescopic star *a* passes	33	45	30
17	29	59	17	24	33	The southern star *η* in *Leo*'s neck passes	33	18	30
						Saturn's *R. Ascens.* 136 58.30			
						Distan. from the N. pole 72.40. 5			
						Thursday Nov. 22.			
16	24	35	16	19	30	The following star to *π* of *Can.* passes	35	21	30
	33	6	16	28	0	*Saturn* passes	34	6	50
	46	27	16	41	21	The said telescopic star passes	33	45	10
17	16	51	17	11	45	The S. star of *Leo*'s neck passes	33	18	30
						Saturn's *R. Ascens.* 136 55.45			
						Distan. from the N. pole 72.39. 0			
						Sunday Dec. 30,			
1	39	43	10	52	42	The bright foot of *Gemini* passes	34	51	30
13	29	25	13	12	24	The preceeding star of *Cancer* to *o* passes	35	4	0
13	49	1	13	32	0	*Saturn* passes	33	30	0
14	39	50	14	22	49	*Lucida Colli Leonis* passes	33	18	40
						Saturn's *R. Ascens.* 135.10. 0			
						Distance from the N. pole 72. 2. 5			
						Anno 1712. *Saturday Jan* 12.			
11	39	52	11	33	6	*ζ* at the tail of *Cancer* passes	32	59	10
12	40	46	12	34	0	*Saturn* passes	33	12	0
12	43	16	12	36	30	The following star of *Cancer* to *π* passes	35	22	20
13	36	44	13	29	58	*Cor Leonis* passes	38	6	55
						Saturn's *R. Ascens.* 134.12.0			
						Distance from the N. pole 71.44.0			
						Saturday, Jan. 19.			
11	45	31	11	36	55	The southern asellus passes	32	17	10
11	58	14		49	38	The south star to *o* of *Cancer* passes	35	3	50
12	11	36	12	3	0	*Saturn* passes	33	2	5
12	20	00		11	24	The telescopic star *b* passes	32	33	20
						Saturn's R. Ascens. 133 37 0			
						Distance from the N. pole 71 34 10			
						Sunday, Jan. 27.			
10	57	50	11	4	31	The southern asellus passes	32	17	10
11	10	54	11	17	35	The north star to *o* of *Cancer* passes	34	48	10
11	21	19	12	28	0	*Saturn* passes	32	50	20
11	32	20		39	1	The telescopic star *b* passes	32	33	25
						Saturn's R. Ascens. 132 58 0			
						Dist. from the N. pole 71 22 20			

Time

Time by the clock. H. M. S.	Correct time. H. M. S.		Dist. from the vertex
		Monday Mar. 31. 1712	
7 8 46	7 7 41	The south asellus passes	32 16 50
7 20 35	7 19 30	*Saturn* passes	32 3 30
7 36 50	7 35 45	The preceeding star of *Cancer* to π passes	35 19 10
7 39 36	7 38 31	The following star to π passes	35 21 20
8 31 51	8 30 46	The S. star in *Leo*'s neck passes	33 18 40
		Saturn's R. Ascens. 130 2 00	
		Dist. from. the N. pole 70 35 30	
		Friday Nov. 7.	
18 17 13	18 22 0	*Saturn* passes	37 57 30
18 26 13	18 31 0	The 40th star of *Leo* in *Catal. Brit.* passes	41 14 10
18 28 35	18 33 22	The 41st passes	40 15 20
		Saturn's R. Ascens. 150 15 30	
		Dist. from the N. pole 76 29 40	
		Monday Nov. 17.	
17 33 19	17 34 59	*Cor Leonis* passes	38 7 20
17 42 20	17 44 0	*Saturn* passes	38 0 50
17 57 48	17 59 28	ρ in *Leo*'s arm-pit passes	40 41 45
18 14 19	18 15 59	*l* in *Leo*'s belly passes	39 24 50
		Saturn's R. Ascens. 150 31 0	
		Dist. from the N. pole 76 33 0	
		Observations of *Jupiter*. *Saturday May* 26, *Anno* 1711.	
12 44 28	12 39 44	The 58th star of *Serpentarius* in *Cat. Brit.* passes	76 12 0
52 40	47 56	*a* of the nebulose star of *Sagit.* passes	75 39 20
13 8 44	13 4 0	*Jupiter* passes	74 37 0
15 43	10 59	The 11th star of *Sagit.* passes	72 5 20
		Jupiter's R. Ascens, 270 19 0	
		Dist. from the N. pole 113 11 50	
		Sunday May 27.	
10 47 24	10 43 2	δ the middle star of *Scorpio*'s fore-head passes	73 12 0
12 57 46	53 24	The telescopic star *c* preceeds *Jupit.*	75 7 50
13 4 22	13 0 0	*Jupiter* passes	74 36 50
11 53	7 31	The 11th star of *Sagit* passes	72 5 20
		Jupiter's R. Ascens. 270 11 0	
		Dist. from the N. pole 113 11 40	

Time by the clock. H. M. S.	Correct time. H. M. S.	*Sunday June* 3. 1711	Dist. from the vertex
10 15 41	10 10 45	The middle star of *Scorpio's* forehead passes	73 12 [illegible]
12 14 11	12 9 15	*b* of the nebulose star of *Sagit.* passes	75 10 20
12 17 9	12 12 13	*a* of the nebulose star of *Sagitt.* passes	75 39 3[illegible]
12 28 56	12 24 0	*Jupiter* passes	74 38 0
13 8 36	13 3 40	The preceeding in *Sagitt.* eye passes	74 29 1[illegible]
		Jupiter's R. Ascens. 269 15 0	
		Dist. from the N. pole 113 12 50	
		Monday June 4.	
10 11 51	10 7 19	The middle star of *Scorpio's* forehead passes	73 12 0
12 10 18	12 5 46	*b* of the nebulose star of *Sagitt.* passes	75 10 20
12 24 32	12 20 0	*Jupiter* passes	74 38 10
13 4 45	13 0 13	The preceeding star to *v* of *Sagitt.* passes	74 29 10
13 5 41	13 1 9	The following star to *v* passes	74 24 50
		Jupiter's R. Ascens. 269 7 0	
		Dist. from the N. pole 113 13 0	
		Saturday June 9.	
9 52 16	9 49 5	The middle star of *Scorpio's* forehead passes	73 11 5[illegible]
11 50 44	11 47 33	*b* of the nebulose star passes	75 10 1[illegible]
12 2 11	11 59 0	*Jupiter* passes	74 38 2[illegible]
12 45 8	12 41 57	The preceeding star to *v* of *Sagitt.* passes	74 29 10
		Jupiter's R. Ascens. 268 25 0	
		Dist. from the N. pole 113 13 15	
		Sunday June 10.	
9 48 22	9 44 38	The mid. star of *Scorpio's* foreh. passes	73 12 0
11 46 51	11 43 7	*b* of the nebulose star passes	75 10 1[illegible]
11 57 44	11 54 0	*Jupiter* passes	74 38 3[illegible]
12 41 14	12 37 30	The preceed. star to *v* of *Sagit.* passes	74 29 1[illegible]
		Jupiter's R. Ascens. 268 16 45	
		Dist. from the N. pole 113 13 25	
		Saturday July 14.	
8 58 10	8 50 4	C the 48th star of *Serpentarius* passes	75 7 20
9 10 32	9 2 26	D the 54th of *Serpent.* passes	72 56 0
9 21 6	9 13 0	*Jupiter* passes	74 37 50
9 40 49	9 32 43	μ in *Sagit.* bow passes	72 31 30
		upiter's R. Ascens. 264 12 30	
		Dist. from the N. pole 113 12 40	

Time

Time by the clock. H. M. S.	Correct time. H. M. S.		Dist. from the vertex
		Sunday July 15. 1711	
8 54 8	8 46 24	C of *Serpentarius* passes	75 7 20
9 6 28	8 58 44	D of *Serpent.* passes	72 56 0
9 16 44	9 9 0	*Jupiter* passes	74 37 50
9 36 46	9 29 2	μ in *Sagit.* bow passes	72 31 35
		Jupiter's R. Ascens. 264 7 45	
		Dist. from the N. pole 113 12 40	
		Anno 1712 *Thursday July* 3.	
12 36 3	12 25 14	σ under *Capricorn*'s eye passes	71 24 40
12 44 6	12 33 17	π in *Capricorn*'s snout passes	70 33 0
12 45 42	12 34 53	ρ of *Capricorn* passes	70 9 50
12 57 49	12 47 0	*Jupiter* passes	71 26 20
13 16 24	13 5 35	The 20th star of *Capricorn* in *Cat. Brit.* passes	71 33 10
		Jupiter's R. Ascens. 306 9 20	
		Dist. from the N. pole 110 0 30	
		Tuesday July 15.	
11 39 34	11 39 33	σ of *Capricorn* passes	71 24 45
55 1	11 55 0	*Jupiter* passes	71 49 20
12 19 53	12 19 52	The 20th star of *Capricorn* passes	71 33 10
24 38	12 24 37	*n* in the mid. of *Capric.* body passes	72 23 25
		Jupiter's R. Ascens. 304 34 35	
		Dist. from the N. pole 110 23 30	
		Wednesday September 17.	
7 32 37	7 31 56	The telescopic star preceeding *Jupit.* passes	72 49 40
7 38 41	7 38 0	*Jupiter* passes	72 51 10
7 42 28	7 41 37	The telesc. star following *Jup.* passes.	73 14 40
		Jupiter's R. Ascens. 299 43 0	
		Dist. from the N. pole 111 25 30	
		Friday September 19.	
7 30 55	7 32 0	*Jupiter* passes	72 50 45
8 19 47	8 20 52	*n* of *Capricorn* passes	72 23 40
		Jupiter's R. Ascens. 299 45 0	
		Dist. from the N. pole 111 25 5	
		Monday October 6.	
6 25 35	6 31 30	*Jupiter* passes	72 39 50
6 36 22	6 42 17	*o* in *Capricorn*'s snout passes	70 56 25
6 46 36	6 52 31	*υ* in *Capricorn*'s neck passes	70 33 30
		Jupiter's R. Ascens. 300 39 0	
		Dist. from the N. pole 111 14 10	

Time by the clock. H. M. S.	Correct time. H. M. S.	Observations of *Mars*. Anno 1711.	Dist. from the verte[x]
		Sunday January 7.	
12 40 35	12 38 56	The S. star of *Cancer* to *o* passes	35 4
12 40 56	12 39 17	The N. star of *Cancer* to *o* passes	34 48
12 58 45	12 57 6	The following star of *Cancer* to *π* passes.	35 21 2
13 27 15	13 25 36	Ψ of *Leo* passes.	36 8 5
13 50 52	13 49 13	*n* the S. star of *Leo*'s neck passes.	33 19
14 0 39	13 59 0	*Mars* passes	34 51 3
14 30 14	14 28 35	*k* the N. star of *Leo*'s belly passes	35 45 4
		Mars R. Ascens. 150.20.0	
		Distance from the N. pole 73.23.35	
		Saturday January 27.	
10 40 59	10 41 30	The 20th star of *Cancer*, *viz.* the 1st to *d* passes	32 14 1
10 50 16	10 50 47	*n* of *Cancer* passes	30 4 3
12 8 29	12 9 0	*Mars* passes	32 14
12 25 27	12 25 58	*n* the S. star of *Leo*'s neck passes	33 19
12 37 59	12 38 30	*Lucida Colli Leonis* γ passes	30 11 5
		The *R. Ascens.* of *Mars* 143.37.0	
		Distance from the N. pole 70.36.0	
		Monday January 29.	
12 1 50	11 59 0	*Mars* passes	31 58 5
12 21 58	12 19 8	*n* of *Leo* passes	33 19 5
12 34 31	12 31 41	γ *Lucida Colli Leonis* passes	30 11 5
		R. Ascens of *Mars* 142.49.30	
		Distance from the N. pole 70.30.50	
		Tuesday January 30.	
10 33 30	10 30 25	The 20th star of *Cancer*, *viz.* the 1st to *d* passes	32 14 1
10 42 35	10 39 30	*n* of *Cancer* passes	30 4 45
11 56 5	11 53 0	*Mars* passes.	31 51 35
12 17 49	12 14 44	*n* of *Leo* passes	33 19 5
12 30 20	12 27 15	γ of *Leo* passes	30 11 5
		R. Ascens. of *Mars* 142.25.0	
		Distance from the N. pole 70.23.35	
		Wednesday February 28.	
8 27 11	8 23 58	μ in the N. foot of *Cancer*	29 5 40
9 2 50	8 59 37	γ the N. *Asellus*	29 0 29
9 31 13	9 28 0	*Mars* passes	30 5 10
10 27 28	10 24 15	*n* of *Leo* passes	33 19 55
10 39 39	10 36 26	γ of *Leo* passes	30 12 5
		R. Ascens. of *Mars* 133.45. 0	
		Distance from the N. pole 68.37.10	

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Time by the clock H. M. S.
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36	8	50
33	19	0
34	51	30
35	45	40
32	14	10
30	4	35
32	14	0
33	19	0
30	11	50
31	58	50
33	19	5
30	11	50
32	14	10
30	4	45
31	51	35
33	19	5
30	11	50
29	5	40
29	0	29
30	5	10
33	19	55
30	12	5

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Time by the clock H. M. S.	Correct time. H. M. S.	*Thursday March* 1. 1711		Dist. from the vertex.
8 22 47	8 19 27	μ of *Cancer* passes		29 5 35
8 58 28	8 55 8	The N. *Asellus* passes		29 0 15
9 26 20	9 23 0	*Mars* passes		30 6 20
10 23 4	10 19 44	*n* of *Leo* passes		33 19 50
10 35 36	10 32 16	*Lucida Colli Leonis* passes		30 12 0
		R. Ascens. of *Mars*	133.37.30	
		Distance from the N. pole	68.38.20	
		Anno 1712. *Monday Nov.* 17.		
18 21 7	18 19 52	The 55 star of *Leo* in *Cat. Brit.* passes		43 45 40
18 25 53	18 24 38	*c* under *Leo*'s belly passes		43 49 40
18 30 14	18 28 59	χ of *Leo* passes		42 35 10
18 43 15	18 42 0	*Mars* passes		43 7 25
		R. Ascens. of *Mars*	165.48. 0	
		Distance from the N. pole	81.39.45	
		Thursday Nov. 20.		
18 13 8	18 6 40	The 55 star of *Leo* passes		43 45 40
18 17 53	18 11 25	*c* The 58 star of *Leo* passes		43 49 50
18 22 14	18 15 46	χ of *Leo* passes		42 35 15
18 38 18	18 31 50	σ in *Leo*'s ham passes		43 52 10
18 40 28	18 34 0	*Mars* passes		43 36 55
		R. Ascens. of *Mars*	167.6.30	
		Dist. from the N. pole	82.9.15	

Observations of the sun
Anno 1711.

Saturday Jan. 6.	
The sun's center passing thro' the plane of the meridian, his remote and S. limb was distant from the vertex	72 31 10
Friday Jan. 26.	
The nearest limb of the sun at noon distant from the vertex	66 35 10
Tuesday Jan. 30.	
The nearest limb of the sun distant from the vertex	65 38 40
Monday June 4.	
The remote limb of the sun, &c.	28 24 18
Saturday July 14.	
The remote limb of the sun, &c.	31 55 20
Tuesday Nov. 20.	
The remote limb of the sun, &c.	73 30 40
Tuesday Dec. 4.	
The remote limb of the sun distant from the vertex	74 59 30

Anno 1712	Dist. from the vertex
Wednesday Jan. 2.	
The remote limb of the sun distant from the vertex	73 17 30
Saturday Jan. 12.	
The remote limb of the sun	71 18 0
Friday March 7.	
The remote limb of the sun	52 31 0
Friday May 9.	
The remote limb of the sun	31 40 50
Tuesday Oct. 7.	
The remote limb of the sun distant from the vertex	61 29 50

Time by the clock. H. M. S.	Correct time H. M. S.	Observations of the moon. Anno 1711 *Saturday May* 19.	Dist. from the vertex
10 22 12	10 18 32	γ of *Scorpio* or the 15th of *Libra* passes	75 32 50
10 50 55	10 47 15	The moon's limb passes its center distant from the vertex	75 38 40
10 52 10	10 48 30	The center of the moon passes, his nearest limb, &c.	75 22 0
11 8 30	11 4 50	*b* of *Scorpio* passes	76 15 20
11 11 10	11 7 30	A of *Scorpio* passes	75 50 55
		The moon's R. Ascens. 229.18.30	
		Dist. from the N. pole 114.13.40	
		Monday November 19.	
16 41 28	16 36 8	The moon's center passes, her remote limb, &c.	37 6 30
17 42 16	16 36 56	The moon's limb passes, her centre distant, &c.	36 52 30
16 46 20	16 41 0	*Saturn* passes	34 8 0
16 59 31	16 54 11	The telescopic star *a* passes	33 45 30
17 29 53	17 24 33	*n* the S. star in *Leo*'s neck passes	33 18 30
		The moon's R. Ascens. 135.41.40	
		Dist. from the N. pole 75.23.20	
		Anno 1712 *Saturday Jan.* 12.	
7 40 50	7 34 0	The middle of a lunar eclipse, when the chord of the part eclipsed was 24.30. The greatest obscuration 8.30. to the north; her diameter 30.48	
11 39 52	11 33 6	*Bayer*'s ζ in *Cancer* passes	32 59 10
12 13 46	12 7 0	The moon's center passes, her remote limb distant, &c.	34 14 10
40 46	12 34 0	*Saturn* passes	33 12 0
43 16	12 36 30	The star in *Can.* following π passes.	35 22 20
13 36 44	13 29 58	*Cor Leonis* passes	38 6 55
		The moon's R. Ascens. 127.25.30	
		Dist. from the N. pole 72.31. 0	

Time by the clock. H. M. S.	Correct time. H. M. S.		Dist. from the vertex
		Thursday March 6.	
7 39 8	7 38 8	The 80 of Gem. in *Cat. Brit.* passes	30 29 30
7 49 42	7 48 42	*l* the 86 of Gem. passes	30 51 45
7 54 45	7 53 45	The moon's limb passes, her cent. *&c.*	30 50 40
7 55 40	7 54 48	The moon's center passes, her nearest limb, *&c.*	30 35 30
8 1 41	8 0 41	μ the ninth of *Cancer* passes	29 4 45
		The moon's R. Ascens. 116.14.40	
		Dist. from the N. pole 69.22.50	
		Wednesday May 7.	
9 47 38	9 46 2	The 70 star of *Virgo* in *Cat. Brit.* passes	66 9 50
9 48 41	9 47 12	The 71 of *Virgo* passes	65 45 30
9 52 51	9 51 15	The 75 of *Virgo* passes	68 7 40
10 9 36	10 8 7	The moon's limb passes, her cent. *&c.*	69 12 30
10 10 43	10 9 7	The moon's center passes, her remote limb, *&c.*	69 28 35
		The moon's R. Ascens. 208. 4.30	
		Dist. from the N. pole 107.45.50	
		Thursday May 8.	
10 52 2	10 50 0	The 8 star of *Libra Cat. Brit.* passes	74 51 50
11 1 35	10 59 33	γ of *Scorpio* or the 15 of *Libra* passes	75 32 30
11 8 2	11 6 0	The moon's limb passes, her cent. *&c.*	73 42 30
11 9 16	11 7 14	The moon's center passes, her remote limb, *&c.*	73 58 20
11 50 33	11 48 31	A of *Scorpio* passes	75 51 10
11 57 34	11 55 32	δ the middle star of *Scorpio*'s fore-head passes	73 12 10
		The moon's R. Ascens. 223.45. 0	
		Dist. from the N. pole 112.16.10	
		Saturday May 10.	
12 18 5	12 16 30	*Scorpio*'s heart *antares* passes	77 10 0
13 3 48	13 2 13	A of *Serpentarius* passes	77 32 50
13 18 13	13 16 38	The moon's center passes, her remote limb, *&c.*	78 1 30
13 19 30	13 17 55	The moon's limb passes, her cent. *&c.*	77 46 5
13 35 25	13 33 50	*p* the first star of *Sagitt.* passes	79 4 20
14 16 8	14 14 33	λ of *Sagitt.* passes	76 57 0
		The moon's R. Ascens. 258. 3.20	
		Dist. from the N. pole 116.20.15	

A large Spleen *and small* Bladder *in a Boy, as also a* Hern
aquosa *in a Man; by Mr.* John Ranby. Phil. Tran
Nº 401, p. 414.

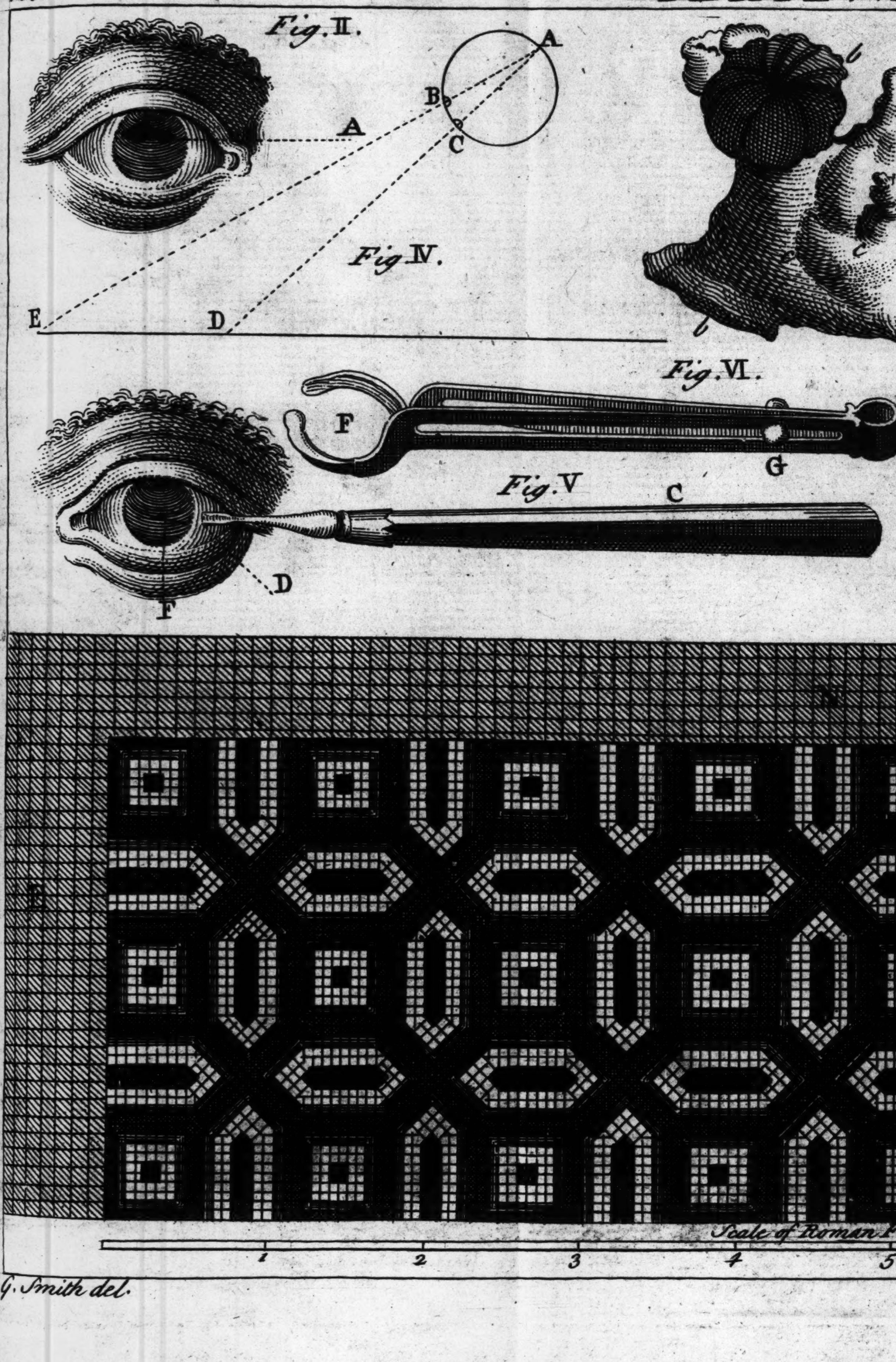
A Boy ten years of age, was kill'd by a blow on the scul
his spleen weigh'd two pounds, and possess'd almost a
the left side of the abdominal cavity. The bladder when di
tended to its largest capacity would not contain an ounce.

A man 25 years of age, who died of a pocky hectic, com
plain'd some days before of a painful swelling in the *Teste*
which he said came the night before. Mr. *Ranby* examin'd i
and finding it to be a *hernia aquosa*, would have punctur'd i
if he had not felt (besides the water) a hard body, which h
could by no means reduce. In a few days the patient died
Upon opening the *Scrotum* and separating the common mem
branes to the *Processus vaginalis*, he found it contain'd abou
four ounces of water, besides a considerable part of the *Omentum*
some portion of which adher'd to the bottom of the cavity an
to the *Albuginea* that immediately covers the *Testes*.

A Roman Pavement *found near* Grantham *in* Lincolnshire
by Dr. Stukely. Phil. Trans. Nº 402. p. 428.

IN *Feb.* 1727-8, plowing in the open fields of *Denton*, abou
two miles and a half from *Grantham*, they lit upon a *Roma*
pavement in *Mosaic* work. It lies partly in the glebe land an
partly in one Mr. *Welby*'s: It has been a very large room, abou
30 foot both ways, as was found by digging in divers places
but being so near the surface, not above a foot and a half deep
and having been plow'd over time out of mind, the greater par
of it is ruin'd and imperfect: Besides several fragments of i
there is only one piece entire, 30 foot long and six foot broad
exceeding pretty, the colours lively, the pattern finely defign'd
as represented by Fig. 1, Plate VIII. which is exactly taken
There are only three colours, viz. white, red and blue; but i
the middlemost or most beautiful part of it, which is but nin
foot long and three foot broad, the white and red is double i
quantity to the blue: In the outermost part, or verge of th
work, there is no variety of colours, but entirely blue; and
that made of much larger squares than the rest. On the eas
and west sides this was six foot broad; on the north but three
The red is form'd out of *Roman* bricks, several fragments o
which were found about the work; the white colour is made o
the common limestone of *Lincolnshire*; the blue, of the ston
tha

PLATE .VI



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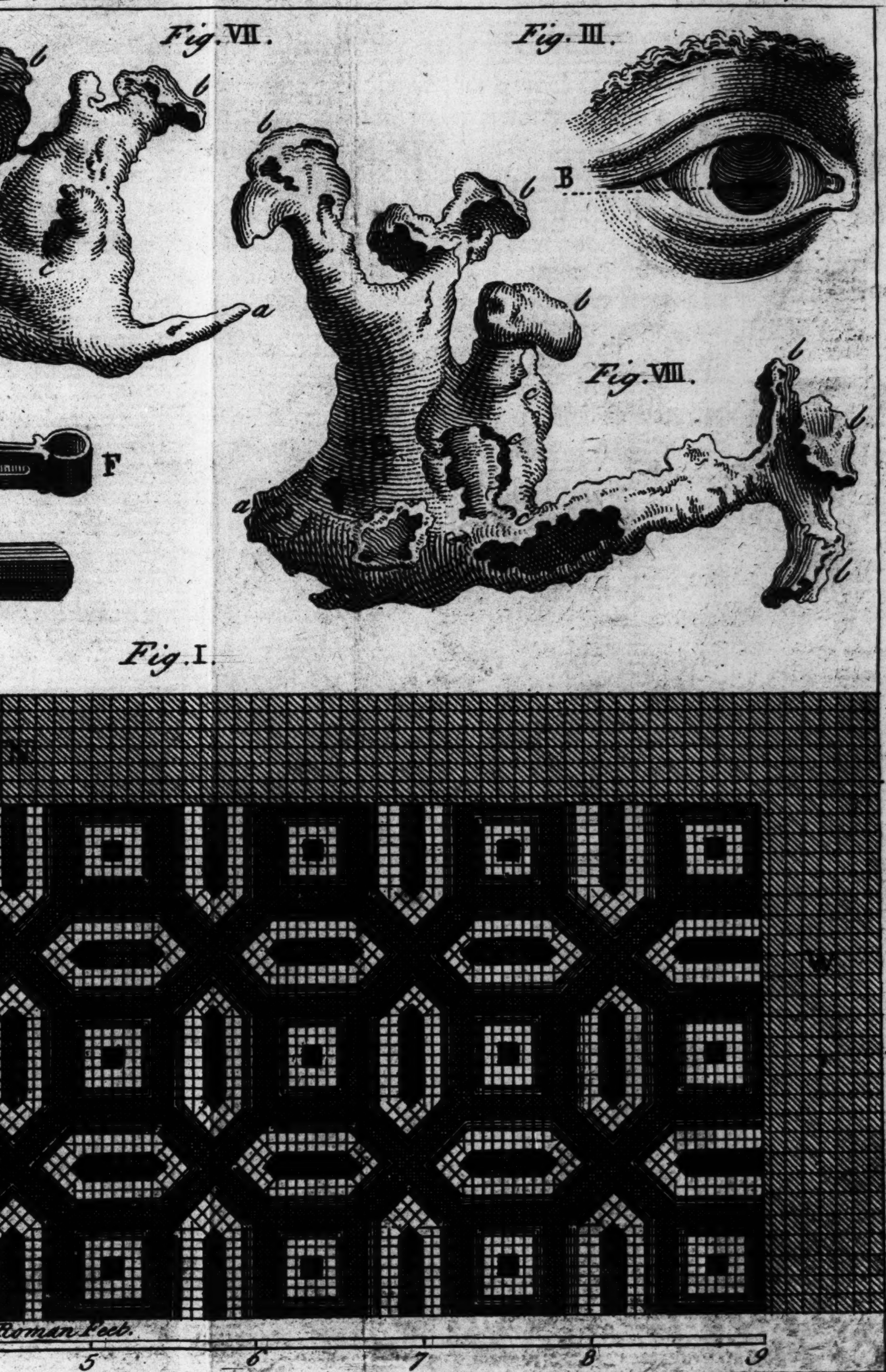

Hulett Sculp.

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that comes from *Benyngton* towards *Newark*, five miles from *Grantham*; and these colours wear well together, and produce a good effect. In digging there were found several parts of the foundations of the walls that terminated this room, and foundations (seemingly) of other adjacent rooms: which foundations were made of the common white stone of the country set on edge side by side, with here and there a bit of *Roman* brick. The building was made parallel to the four quarters of the heavens. In digging there were found some human bones, and Dr. *Stukely* took several bones of a hand, which, probably, belonged to some unfortunate person, kill'd in the ruins, or when the house was demolish'd.

The Oeconomy *of the* Roman Times *near* Grantham; *by Dr.* Stukely. Phil. Transf. N° 402. p. 430.

THREE miles south of *Grantham* is *Great Paunton*, a village situated in a pleasant valley, where the beginnings of *Witham* river are collected from several springs a little higher up. Hard by, upon the edge of the hill, runs the great *Roman* road, call'd *Hermen-street*: This village Dr. *Stukely* in his itinerary asserted to be the *Causennis* in *Antoninus*'s itinerary, in settling which Commentators have found a difficulty. The *terms*, or two towns between which it is placed in *Iter* V. with the distance of the miles, sufficiently establish the situation, and they are well known. Thus *Durobrivas*, *Causenni*, *m. p.* XXX. *Lindum* m. p. XXVI. For *Paunton* is 30 *Roman* miles from *Durobrivæ*, or the passage of the *Hermen-street* over the *Nen-river* above *Peterborough*, and 26 miles from *Lincoln*.

Several *Mosaic* pavements were discover'd at *Paunton*; and, undoubtedly, it was the station upon the *Hermen street* between *Brigcasterton*, 12 *Roman* miles off, and *Ancaster* 7, which were likewise *Roman* stations, and wall'd about; but now their names are lost. The *Romans* inhabited in great numbers all about *Paunton*, particularly at *Kirkstoke*, where several antiquities have been discovered: In like manner the Dr. has seen several *Roman* coins dug up at *Strawston*, in the possession of Captain *Hacket*; who likewise said, that subterraneous vaults were found there, and near it is the place where the pavement was discover'd. Between *Paunton* and *Grantham* a road passes the river at *Salter*'s *ford*; which road is call'd *Salter*'s *gate*, and much frequented. The Dr. believes it has continued ever since the *Roman* times, being the passage from *Holland* in *Lincolnshire* by *Brig-end causey* (a *Roman* work) to all the towns upon the *Fossway* in *Nottingham-*

tinghamshire and *Leicestershire*: For, along this way they carried salt, made by the sea-side, to those midland parts, both in the *Roman* and *Saxon* times: The said pavement stands within a bow-shot of this road, upon very high ground and seems to have been a summer *villa*, or pleasure house, where on the one side they were entertain'd with the sight of travellers going on the road; and on the other side they commanded a most noble prospect northwards, of a vast extent. When we are upon the spot, we may easily perceive the reason, why they fix'd it in that very point, and it is the most delightful place that can be imagined for a summer retreat. It is placed just at the head of a *convallis* or lesser valley, that falls down into the large valley of *Denton* underneath, from which *Denton* has its name, signifying the *town in the valley*. This valley is exceeding beautiful, and running northwards, must needs be very pleasant and cool in the summer time. The *villa* had this farther advantage in its situation, that it commanded a prospect between the opening or gap that appears in this place, between *Barrowby* and *Wolsthorp* hills; which leads the eye into the boundless prospect of the vale of *Belvoir*. From hence you see *Newark*, the *Trent*, *Kelham* house, and *Kelham* park, *Southwell Minster*, the forest of *Sherwood*, &c. besides the neighbouring castle of *Belvoir*, where in those times was a *Roman* exploratory camp, as also the *Roman* camp of *Hunington*: Insomuch that we may well commend the wisdom and good taste of the builder, who contriv'd it so well both for security and pleasure. For air, the country hereabouts has always (and deservedly) been reckoned the *Montpelier* of *England*; for water, wood, heath, and prospect, it may be thought the *Frescati*.

Reflections on M. De Lisle's *Comparison of the bigness of* Paris *with that of* London, *and several other Cities; by Mr.* Peter Daval. Phil. Trans. N° 402. p. 432.

IN the *Memoirs of the Royal Academy of Sciences* at *Paris* for the year 1725, M. *De Lisle* in the account of his method of making an exact plan of *Paris*, and comparing it with *London*, and other cities, first shews by what means he proceeded in determining the true situation of the several places in *Paris*: After which he explains his manner of drawing a true meridian line thro' that city; whereby he was enabled to divide it by meridians and parallels, as is practised in a general map: And then he goes on in the following words.

'I traced the parallels from 15 to 15 seconds, and the 'meridians from 20 to 20 seconds: And as under the parallel 'of *Paris*, 15 degrees of latitude are equivalent to 20 of 'longitude, and the like holds true of minutes and seconds; 'by allowing five seconds more to the intervals of the meri-'dians, than to those of the parallels, I form'd perfect 'squares.'

He says, the chief use he intended to make of these squares was to compare the magnitude of *Paris* with that of *London*, and gives an account of what method he took to procure a just plan of this city, which he reduced to the same scale as that of *Paris*, and proceeds thus

'In like manner I traced upon it squares from 15 to 15 'seconds of a great circle, and then I was prepar'd to compare 'the bigness of the two cities.

'The result of this comparison is, that *Paris* contains 63 'of these squares, which makes for its superfices 3538647 'square toises, and that *London* contains only 60 of those 'squares, or 3370140 square toises.'

And from hence he concludes, that *Paris* is one twentieth part bigger than *London*, tho' he says he has excluded several gardens, contain'd within *Paris*, out of this mensuration, which would have made it bear a still greater proportion to *London*.

Upon reading this account of M. *De Lisle*'s, it immediately occurr'd to Mr. *Davall*, that the method the former has here taken of comparing the magnitudes of *Paris* and *London*, and whence he infers, that the first of these cities is one twentieth part bigger than the latter, is founded on a false supposition, *viz.* that under the parallel of *Paris*, 20 degrees of longitude are equal to 15 of latitude; and consequently, that by drawing meridians from 20 to 20 seconds, and parallels from 15 to 15 seconds, the figures form'd by their intersection will be perfect squares: For, the equator and its parallels are to each other as the sines of their respective distances from the poles: Whence, as the radius, or sine of 90 degrees: is to the sine of the distance of any parallel from the pole, or co-sine of its latitude: : So is a degree of any other part of the equator, or of any great circle: to the like part of the given parallel. Therefore, taking the mean latitude of *Paris* at 48° 51', the proportion of the degrees of a great circle to those of the parallel of *Paris* will by a table of sines be found to be as 1 to .6580326: Whereas, according

to

to M. *De Lisle*, that proportion is only as 20 to 15, or as to .75: The figures therefore, which M. *De Lisle* calls square are not such, but rectangles, whose longest side containin 15 seconds of a great circle, bears the same proportion to th shortest, containing 20 seconds of the parallel of *Paris*, a .75 does to .658, &c. or nearly, as 8 to 7: And the interval which he ought to have allow'd to the meridians, to mak perfect squares of these figures ought to have been $\frac{1}{6}$}8, &c seconds, are nearly $22'' \frac{4}{5}$ or $22''\ 48'''$ of the parallel of *Paris*.

Now M. *De Lisle* says, these figures are perfect squares and he has computed them as squares, whose side was $15''$ o a great circle; for, he says, *Paris* contains 63 of thes squares, which makes 3538647 square toises, which la number being divided by 63, the quotient 56169 will be th number of square toises contain'd in each square, who square root gives 237 toises for the side of each square, whic is just $15''$ of $2\frac{1}{4}$ o, or a degree of a great circle.

M. *De Lisle* hath, therefore, by this account, made th superficial content of each rectangle, and consequently, of th whole city of *Paris*, too great by near one seventh: To con firm which beyond contradiction we have M. *De Lisle*'s ow testimony, who in the plan of *Paris* he himself has drawn an published; and which he refers to in this very account, ha not made squares of the above-mentioned figures, but ha given to their respective sides the proportion of 8 to 7, whic is as near the truth, as can well be express'd by lines, in plan of no larger a scale than this.

Now in the account we have been considering, M. *De Li* says himself, that in his measuring of *London* he drew square whose sides contain'd 15 seconds of a great circle, and o these he says *London* contains 60.

Therefore, to compare *London* with *Paris*, we ought fo the foregoing reasons to make an abatement out of the 6 rectangles which *Paris* contains, nearly in the proportion o 8 to 7: But because that is somewhat greater than the tru one; let us make such abatement only in the proportion o 9 to 8, which is considerably less than the just one: By whic abatement the number of squares, whose side is $15''$ of great circle, contain'd in *Paris*, will be reduced from 63 t 56. And consequently, according to M. *De Lisle*'s way o measuring, the magnitude of *London* will be to that of *Par* as 60 to 56, or as 15 to 14; or *London* will be one fourteent part bigger than *Paris*. But to determine what proportio

thes

these two cities really bear to each other, requires a more exact mensuration of *London* than any we hitherto have; and whoever would undertake it, Mr. *Davall* thinks, he cannot follow a better method than what M. *De Lisle* has taken, and he would advise him to consult the account on which the foregoing reflections are made, which he may find in the *Memoirs of the Royal Academy of Sciences* for the year 1715, p. 48.

An Aneurysm *of the* Aorta; *by Dr.* Dodd. Phil. Transf. N° 402. p. 436.

AN *Aneurism*, undoubtedly, is a tumour arising from some disorder in an artery; but what that disorder is, or whence it arises, is not so well agreed; the accounts, which are given of it, being widely different and uncertain. The name seems to imply, that it is a dilatation of the vessel; but *Galen* describes it to be a tumour, which arises, not from any dilatation or relaxation of an arterial vessel, and the blood therein contain'd; but from an extravasation of the blood from some rupture of the artery: Agreeable to this are the opinions of all the rest of the ancients, as likewise of the *Arabians*, who borrow'd most of what they have from them. *Furnelius*, as it is said, is the first who maintained, that the artery was only dilated, and not burst in an *Aneurysm*, and that the blood was contain'd within the coats of it, as it is within those of the vein in a *varix*; which, therefore is call'd by some ἡ φλὲψ ἀνευρυσμενη. *Sennertus* makes it to be a dilatation, not of both the coats, but of the outer one only, the inner or muscular one being first bursten or broken; and he is followed herein by most of those who have succeeded him, excepting *Wiseman* and some others, who tell us, that it is nothing but an extravasation of the blood, bursting thro' the coats of the arteries into the interstices of the muscles, and there forming a tumour suitable to the cavity it finds, the artery remaining undistended or undilated all the while; and that in all these *Aneurysms*, which have come to be examin'd, both the coats of the artery have constantly been found broken.

This being the state of opinions with relation to an *Aneurism*, Dr. *Dodd* had an opportunity of examining farther into it, by means of a patient, who was taken into St. *Bartholomew*'s hospital. She was about 34 years of age, and of a good constitution; but there was a tumour bigger

than one's fiſt, which began from the upper part of the *ſter-num*, between the origins of the *muſculi Maſtoidæi*, and extended itſelf to the *pomum Adami*, almoſt up to her chin, and poſſeſs'd all the breadth between the two carotid arteries. The account that ſhe gave of the occaſion of it was, that her huſband being a paſſionate man, took her by the throat one day, as ſhe was crying out on ſome occaſion or other, and griped her ſo hard, as almoſt to throttle her: She was then with child, and immediately perceiv'd ſomething of a pain a little above her heart; and a few days after there appeared a tumour, about the bigneſs of the tip of her finger, just above the *ſternum*; and ſo continued without increaſe or pulſation till ſhe was brought to bed, when it began to be enlarged, upon her having a hard labour; agreeable to what practitioners have obſerv'd, that accidents of this nature often happen to women in labour. This was about four years before, and from that time it had continued gradually increaſing, till it was arriv'd to almoſt the higheſt pitch of extenſion; and ſhe had all along been troubled with a palpitation, pain and ſtraitneſs within the *thorax*, great interruptions in her reſt, and frequent ſinkings, together with a conſtant beating along the cheſt up to the tumour; in which there was likewiſe a pulſation correſponding to the regular pulſe, ſhaking the tumour at every ſtroke, and manifeſt to the eye as well as the touch: Notwithſtanding this ſhe was otherwiſe hearty, had her *menſes* regularly, had a good appetite, and for the moſt part was chearful and lively; and never more ſo than juſt before the fatal period of the tumour. The *apex* of the tumour, which was towards the middle, i.e. the prominent part thereof, was beginning to mortify, thro' an over diſtenſion; and the common outer integuments were the firſt that ſeemed to ſuffer: But the diſtention continuing, the mortification increas'd, and was quickly communicated in like manner to the outer coat of the artery, which, therefore, ſlough'd off, as well as the other integuments, and being at length wore away, juſt at the extremity, it made a ſudden aperture, about twice the bigneſs of a gooſe-quill: The blood inſtantly guſh'd forth, as from a ſtream or torrent, and the patient died in leſs than a minute.

Upon opening the body, they began from the heart, in which there was little remarkable, only that the left ventricle was ſomewhat larger, as were likewiſe the *columnæ carneæ*, tho' in a natural ſtate. There was little obſervable in the *aorta*

ſo

self, till they came to the curvature, upon the upper side of which was the basis of the tumour, forming a cylindrical stem four inches long while in the cavity of the *thorax*; but extending itself into a circular form of a larger dimension, when it became external. Upon opening the under part of the *aorta*, opposite to this basis, and carrying the incision throughout its whole extent in the *thorax*, the trunk retain'd its usual form, and dimensions, and was not at all dilated; but in the upper part above describ'd, just on this side the orifice of the right subclavian artery (which was nearer than usual to the orifice of the left carotid) there was a preternatural circular aperture, half an inch in diameter. Upon dividing this aperture, and carrying on the incision to the *apex* of the tumour, its whole internal substance appear'd. The edges of the aperture at the basis of the tumour were hard, and almost cartilaginous, and seemingly the remains of thick and fleshy fibres; which upon a narrower inspection they really appear'd to be, *viz.* the broken fibres of the inner, or what is commonly call'd the muscular coat of the artery; which terminating here, the tumour immediately increas'd to two inches in diameter, and continu'd of that dimension, till it came out of the neck, between the clavicles; but then extended itself circularly to a diameter upwards of three inches, the covering of which was nothing else but the outer coat of the same artery, all along dilated from the basis, even to the extremity of the tumour. The cavity was for the most part filled with a sort of *polypus*, or *sarcoma*; in which, nevertheless, were three sinus's, or passages, that were kept open by the constant influx of the blood, and communicated near the *apex* with each other (that in the middle being the largest) terminating in one towards the extremity of the tumour, not far from where it broke.

Observations on Aneurysms *in general, and on the foregoing in particular; by Dr.* Nicholls. Phil. Transf. N° 402. p. 440.

AN *aneurysm* is by all authors defined to be a soft circumscribed tumour, in which there is a sensible pulsation, contemporary with that of the artery, to which it adheres. As it is certain, that any tumour of what kind soever, lying on, or adhering to any considerable artery, must necessarily be moved by every pulsation of such artery; so this pulsation (unless understood in such a manner as Dr. *Nicholls* shall hereafter explain) can nowise be admitted as the true diagnostic, whereby

to specify the difference between this kind of tumour and any other. An *aneurysm* is found most commonly to succeed falls, vomitings, labour-strains, and such other motions, or indispositions of the body, as by compressing the large branches of an artery, anyways stop the progressive motion of the blood. It is obvious, that as the section of the artery above the compressure must in its natural state be sometimes very incapable of containing at once the whole quantity of blood, which ought only to have passed thro' it successively; and as the force of the heart may frequently exceed the resistance it may meet with from the coats of the artery: So the consequence of such a stop to the progressive motion of the blood, may occasion either a rupture of the artery, or a distension of the artery without a rupture; or a rupture of the internal coats of the artery, and a distension of its external coat. A rupture of the large branches of the *aorta* must necessarily be attended with such plentiful effusions of blood, as to occasion immediate death; while the capillaries may be burst without any other injury, but a slight *ecchymosis*, and the tumour formed by the effusion from them will be diffused and superficial. A rupture of the mean branches (such as descend between the *tibia* and *fibula*, the *radius*, and *ulna*, &c.) will be attended with a considerable effusion of blood; but as the blood will find a passage between the interstices of the muscles, it will never form a circumscribed tumour. However, the effusion being continued *per saltum* thro' the ruptured artery, will give a faint pulsation; and consequently, some resemblance of the *aneurysm*; for which reason it is called by some surgeons a bastard *aneurysm*.

Whether or no an *aneurism* be a tumour formed by the dilatation of the artery, or by a rupture of the internal coats of the artery, and a distension of the external, has for some time been a matter of great dispute; each party protesting (perhaps too unjustly) against the possibility of the other's opinion. As to the possibility of an artery being dilated, it stands supported by reason and autopsy. We find the uterine arteries constantly increased in thickness and diameter, in proportion as the *uterus* is distended; and many cases of palpitations of the heart have been attended with considerable dilatations of the *aorta*; instances of which Dr. *Nicholls* himself has observed both in human and brute subjects. Such a dilatation will necessarily follow a constant, or frequent pressure on any part of the *aorta*, provided such pressure do not entirely stop the progressive motion of the blood thro' the *aorta*: But on the other hand, such a dila-

a dilatation will always retain ſomewhat of the form of the artery. The reſiſtence will not be every way equal, as in the extravaſate tumours; becauſe the quaquaverſal preſſure of the blood will be controlled by the preſſure on the artery, and the reſiſtence from the coats of the arteries; ſo as neceſſarily to form a cylindroid; and the conſequence of ſuch a dilatation cannot (if conſidered abſtractedly from its preſſures) be worſe (if ſo bad) than from a varicoſe vein.

Again, they who take an *aneuryſm* to be a rupture of both coats of the artery, oppoſe the opinion of ſuch, as imagine the internal coat to be ruptured, and the external coat to be diſtended, by comparing the two coats in queſtion, and urging, that as the internal coat is ſo much thicker than the external, it ſeems impoſſible, that the laſt ſhould be ſufficient to reſiſt a force capable of deſtroying the firſt. Were theſe two coats ſimilar as to their ſtructure, we might then compute their ſtrength by their thickneſs, and this argument would be of much greater force than at preſent it can be; becauſe the internal coat being compoſed of annular *faſciculi*, whoſe ſides have but a very weak coheſion, their power of reſiſting will not be meaſurable by the ſtrength of thoſe *annuli*, but by the force with which they adhere laterally. And on the other hand, the external coat being compoſed of fibres equally interwoven, and of a quite different compoſition, it may either exert a greater reſiſtence, or be capable of much greater dilatations than the internal coat. But that autopſy may evince the truth of this difference in the ſtrength of theſe coats, it will be found by any one who pleaſes to try the experiment (which was accordingly tried before the *Royal Society* to their ſatisfaction) that by blowing into the pulmonary artery, the internal coat will ſoon burſt, and the external coat form itſelf into *aneuryſmous* tumours.

Dr. *Nicholls* upon conſidering all this, and having, by order of the *Royal Society*, both privately and publickly examined the *aneuryſm* mentioned by Dr. *Dodd*, finds it to be round like other extravaſate tumours, unleſs when controll'd by any notable preſſure; and that the *ſacculus* does not divide into coats, as the artery from whence it ariſes does, he is apt to think that this *aneuryſm* is a tumour formed by the blood's being forced thro' the ligamentous, or what is called the muſcular coat, and diſtending the membranous or outer coat. And becauſe the *impetus* of the blood will perpetually preſs, as it were, thro' the aperture into the tumour, and be again (at leaſt in part) returned by

by the elaſticity of the external coat; ſuch a tumour there-fore will rather have a pulſatile dilatation than a pulſation fo[r] it true diagnoſtic.

A ſurpriſing ſhoal of Pumice-ſtones *found floating on th[e]* Sea; *by Mr.* John Dove. Phil. Tranſ. N° 402. p. 444.

ON the 22d of *March* 1724-5 at noon Mr. *Dove* being i[n] Lat. 35° 36′ S. and Long. 4° 9′ W. with variation 3° 16 W. diſcovered ſeveral pumice-ſtones floating on the ſea; but no[t] expecting any ſuch thing at that diſtance from land (the iſland[s] *Triſtan* and *d' Acunha* being the neareſt, which he judged to bea[r] from W. 9° 10′ S. diſtance 186 leagues) we were in diſput[e] what it might be; when about 1 *p. m.* we took up a piece in [a] bucket (the ſhip going then but three knots) which confirme[d] Mr. *Dove*'s opinion of its being pumice-ſtones. Toward[s] night it was ſpread all round us, as far as we could ſee: Th[e] wind being variable from N. by E. to E. we ſtood to the eaſt-ward: Towards morning the wind veering to the northward w[e] ſteered E. S. E. The pumice-ſtones were very thick, in drift[s] lying N. N. E. and S. S. W. and extended out of ſight at th[e] maſt's head, increaſing as we ran to the eaſtward. To the 23 at noon Mr. *Dove* made the courſe S. 38° 30′ E. diſtance 7[…] miles; Lat. by obſervation 36° 35′ S. Long. 3° 24′ W. fro[m] the meridian of *London*. On the 24. clear weather and freſ[h] gales, variable from N. E. by N. to N. by W. with a lon[g] ſwell from the eaſtward. We continued our courſe E. S. E. 140 miles, the pumice-ſtones being thicker: So that from th[e] 23. at noon, till 4 next morning, ſome of the drifts were abou[t] a cable's length broad, and ſo thick, we could ſcarce ſee th[e] water between them; and there was much the ſame breadth be-tween the drifts, with ſeveral pumice-ſtones interſperſed. To-wards noon Mr. *Dove* found the pumice-ſtones ſomewhat thinne[r] Lat. 37° 35′ S. and Long. 1° 4′ W.

On the 25. from noon till 2 next morning a freſh gale at N and N. by E. afterwards a little wind from W. to N. W. with [a] N. E. ſea: We ſteer'd E. by S. 101 miles. In the evening th[e] drifts were near as large as above, but towards morning the[y] decreaſed much; ſo that about noon we were clear of theſe pu-mice-ſtones, ſeveral of which were as big as a man's head. We ſailed 317 miles ſince we firſt diſcovered them. They lay juſt in the track for ſhips outward bound, and there is no ac-count of them before: But all the ſhips that went out the ſam[e] year and ſince (that go ſo far to the ſouthward) have fallen in

wit[h]

with them. In the morning we tried the current, but found none, and no bottom at 130 fathoms. At noon Lat. 37° 54′ S. Long. 0° 38′ E. The following evening variation 6° 12′ W. At noon Mr. *Dove* judged *Tristan*, and *d'Acunha* then bore from them W. 3° 39′ N. Distance 256 leagues, supposing it to lie in Lat. 37° 5′ S. and Long. 15° 38′ W.

Observations made by a young Gentleman, *who was Born* Blind, *or lost his Sight so early, that he had no remembrance of ever having seen, and was couched between* 13 *and* 14 *Years of Age; by Mr.* Cheselden, Phil. Trans. N° 402. p. 447.

THO' we say of the Gentleman that he was blind, as we do of all who have ripe cataracts; yet they are never so blind from that cause, but that they can discern day from night; and for the most part do in a strong light distinguish black, white and scarlet; but they cannot perceive the shape of any thing: For, the light by which these perceptions are made, being let in obliquely thro' the aqueous humour, or the anterior surface of the crystalline (by which the rays cannot be brought into a *focus* upon the *retina*) they can discern in no other manner than a sound eye can thro' a glass of broken jelly, where a great variety of surfaces so differently refract the light, that the several distinct pencils of rays cannot be collected by the eye into their proper *foci*: Wherefore, the shape of an object in such a case, cannot at all be discerned, tho' the colour may; and thus it was with this young Gentleman, who tho' he knew these colours a-part in a good light; yet when he saw them after he was couched, the faint ideas he had of them before were not sufficient for him to know them by afterwards; and therefore he did not think them the same, which he had before known by those names. Now he thought scarlet the most beautiful of all colours, and of others the most gay were the most pleasing; whereas the first time he saw black, it gave him great uneasiness, yet after a little time he was reconciled to it; but some months after, accidentally seeing a *Negro* woman, he was struck with great horror at the sight.

When he first saw, he was so far from making any judgment about distances, that he thought all objects whatever touch'd his eyes (as he express'd it) as what he felt, did his skin; and he thought no objects so agreeable as those which were smooth and regular, tho' he could form no judgment of their shape, or guess what it was in any object that was pleasing to him: He did

did not know the ſhape of any thing, nor any one thing from another, howſoever different in ſhape or magnitude: But upon being told what things were, whoſe ſhape he before knew from feeling, he would carefully obſerve, that he might know them again; but having too many objects to learn at once, he forgot many of them; and (as he himſelf ſaid) at firſt he learned to know, and again forgot a thouſand things in a day. One particular only (tho' it may appear trifling) Mr. *Cheſelden* relates, *viz.* that having often forgot which was the cat, and which the dog, he was aſham'd to aſk; but laying hold of the cat (which he knew by feeling) he was obſerv'd to look at her ſtedfaſtly; and then ſetting her down, ſaid, ſo puſs! I ſhall know you another time. He was very much ſurpriz'd, that thoſe things he had liked beſt, did not appear moſt agreeable to his eyes, expecting thoſe perſons would appear moſt beautiful that he lov'd moſt, and ſuch things to be moſt agreeable to his ſight that were ſo to his taſte. We thought he ſoon knew what pictures repreſented, which were ſhewn him; but we found afterwards we were miſtaken; for, about two months after he was couch'd, he diſcover'd at once they repreſented ſolid bodies; when to that time he conſider'd them only as party-colour'd planes, or ſurfaces diverſified with a variety of paint; but even then he was no leſs ſurpriz'd, expecting the pictures would feel like the things they repreſented; and was amazed when he found thoſe parts, which by their light and ſhadow appear'd now round and uneven, felt only flat like the reſt; and aſked which was the lying ſenſe, feeling or ſeeing?

Being ſhewn his father's picture in a locket at his mother's watch, and told what it was, he acknowledged a likeneſs, but was vaſtly ſurpriz'd; aſking, how it could be, that a large face could be repreſented in ſo little room, ſaying, it ſhould have ſeemed as impoſſible to him, as to put a buſhel of any thing into a pint.

At firſt he would bear ſeeing but very little, and the things he ſaw, he thought extremely large; but upon ſeeing things larger, thoſe firſt ſeen he conceiv'd leſs, never being able to imagine any lines beyond the bounds he ſaw: The room he was in he ſaid he knew to be but part of the houſe; yet he could not conceive that the whole houſe could look bigger. Before he was couch'd he expected but little advantage from ſeeing, worth undergoing an operation for, except reading and writing; for, he ſaid, he thought he could have no more pleaſure in walking abroad than he had in the garden, which he could do very ſafely

and

nd readily; and even blindneſs he obſerv'd had this advantage, hat he could go any where in the dark much better than thoſe who can ſee, and after he had ſeen, he did not ſoon loſe this uality, nor deſire a light to go about the houſe in the night. He ſaid every new object was a new delight; and the pleaſure was ſo great, that he wanted ways to expreſs it: But his gratitude o his operator he could not conceal, never ſeeing him for ſome ime without tears of joy in his eyes, and other marks of affec-ion: And if he did not happen to come at any time when he was expected, he would be ſo griev'd, that he could not forbear rying at his diſappointment. A year after firſt ſeeing, being arried upon *Epſom Downs*, and obſerving a large proſpect, e was exceedingly delighted with it, and call'd it a new kind f ſeeing. And now being couch'd of his other eye, he ſays, hat objects at firſt appear'd large to this eye, but not ſo large s they did at firſt to the other; and looking upon the ſame ob-ct with both eyes, he thought it look'd about twice as large as ith the firſt couch'd eye only; but not double, that we could nywiſe diſcover.

n Explication of the Inſtruments uſed in a new Operation on the Eyes, *by the ſame.* Phil. Tranſ. N° 402. p. 451.

FIG. 2. and 3. Plate VIII. repreſent two eyes, on which a new operation was performed, by making an inciſion thro' e *iris*, which had contracted itſelf in both caſes ſo cloſe, as leave no pupil open for the admiſſion of light.

The perforation in the eye Fig. 2. was made a little above e pupil, the cloſing of which enſued upon the putting down cataract, which, Mr. *Cheſelden* not knowing how low it might lodged, made the inciſion a little higher than the middle, ſt any part of it ſhould lie in the way.

The eye, repreſented Fig. 3. was one he couch'd not long efore, where the patient had been blind but a few years. At ſt he thought every object farther from him than it really as; but he ſoon learned to judge of the true diſtance, the uſe of which Mr. *Cheſelden* endeavours to explain by Fig. 4. which let the circle A B C repreſent the eye; A the place here an image thro' the natural pupil B was repreſented from e place E; now the artificial pupil being at the place C, the ject at D is now painted at the place A, where the object E as alſo to be perceived; therefore, it was, he ſuppoſes, that e patient miſtook the place D for the place E.

Fig. 5. represents a sort of needle with an edge on one side, which being pass'd thro' the *tunica sclerotis*, is then brought forwards thro' the *iris* a little farther than F. This done, he turns the edge of the needle, and cuts thro' the *iris* as he draws it out; the handle of this needle is half black, and half white, which tho' it be not of much use in this operation, is very much so in couching needles, we being thereby able to judge of their position, when we do not see them.

Fig. 6. represents an instrument to keep open the eye-lids; G is a bit of iron, which as it is moved backward, or forward, the instrument opens and closes.

Observations on Dissecting the Body of a Person troubled with the Stone; *by Mr.* John Dobyns. Phil. Trans. N° 402. p. 452.

MR. *Lawrence*, a Gentleman about 40 years of age, had for near 20 years a complaint in his kidneys; making bloody urine upon any extraordinary motion, but free from the great pain, and all other symptoms usually attending nephritic cases. However upon opening the patient's kidneys after his death, there was in each a stone of an extraordinary size and figure, besides 100 smaller: The two following figures represent the two largest.

Fig. 7. Plate VIII. shews one of them denudated as taken from the *pelvis* of the right kidney; *a* that part which had branched into the *ureter*, and totally obstructed its channel; *b*, *b*, *b* the eminent parts of it; *c*, *c*, *c* that part which filled the capacity of the *pelvis*.

Fig. 8. represents the stone taken from the left kidney; *a* that part which had protruded itself into the upper part of the *ureter*; but did not entirely plug it up; so that the urine had a passage; *b*, *b*, *b*, *b* the eminent parts which branched into the *fistulæ membranaceæ*; *c*, *c*, *c*, *c* the body of the stone which lay in the *pelvis* of this kidney.

An Aurora Borealis *observed by several Persons.* Phil. Trans. N° 402. p. 453.

M. *Gaudin* in a letter from the Observatory at *Paris*, dated *Oct.* 20. 1726, N. S. writes, that he observed it first at half an hour past 7 o'clock in the evening, forming at that time a luminous arch (with another somewhat darker under it) which extended itself almost from sun-set to moon-rise, and was raised above the horizon about 25 degrees; from whence there shot out

out from time to time luminous ſtreams about 10 degrees above it. At half an hour paſt 8, the number of theſe ſtreams vaſtly increaſed, covering all the heavens, excepting the height of 20 degrees oppoſite to it: But towards the zenith there remained a circular ſpace which was never covered by them, tho' there wanted not a conſtant ſucceſſion. Theſe appearances continued very ſtrong till half an hour after 10; when they began to abate, and entirely diſappeared about 2 o'clock in the morning.

M. *Maraldi*, in a letter, dated at *Thiers Oct.* 20, 1726. N.S. two leagues to the ſouth of *Paris*, ſays it began there about half an hour paſt 6 o'clock in the evening, with a conſtant uniform light in the north; ſoon after which there appear'd 3 or 4 luminous arches one over another, from whence iſſued a great number of rays, which ſhot up a conſiderable height above the horizon. At 8 o'clock theſe rays darted quite up to the zenith; half an hour after which they were much increas'd, ſpreading with ſtrong undulations all over the ſky; and all terminating in the zenith formed a ſort of *cupola* there. The concluſion he did not obſerve.

S. *Quaranbotti* writes from *Treggiaia, Oct.* 20. 1726. N. S. that he firſt obſerved it a little before 8 o'clock in the evening, when it extended itſelf along the north horizon about 80 degrees, and reach'd above it about 8 degrees. Some time after, the luminous emiſſions began to riſe perpendicularly, and continued from time to time ſo to do, from 9 till 11 o'clock. About 10 it enlarged itſelf 15 degrees farther eaſt, and extended under the laſt ſtar in *urſa major*. At 11 it vaniſhed.

An anonymous account from *Florence* informs us, that it was firſt obſerved there at half an hour paſt 6 o'clock in the evening, with a clear expanded light, poſſeſſing all the ſpace between the north-eaſt and north-weſt. At 7 it divided itſelf into ſeveral ſpherical triangles near the horizon, which half an hour after united into one large triangle, whoſe baſe was near the horizon, and extended 20 degrees to the weſt from the north-pole, and whoſe vertex reach'd up to *urſa minor*. This continued about half an hour, and then diſappeared; but at 10 o'clock it returned more conſpicuouſly, forming about the pole a large column which was raiſed 30 degrees above the horizon. From this time it emitted lucid undulations till midnight, when it entirely diſperſed. The author afterwards takes notice that the ſame was obſerved at *Milan* and *Bologna*;

the accounts from whence agree, that none of the streams reach'd beyond the zenith.

S. *Manfredi* writes from *Bologna Jan.* 3. 1726-7, that he did not observe this phenomenon himself, but was informed that it was seen every where in the *Campagna di Roma*, as far as *Pesaro* and *Fano*.

Dr. *Burman*, in the *Act. Liter. Suec. Trimest. prim.* 1727, takes notice, that tho' this meteor was seen in *Germany*, *Poland*, *Switzerland*, *France* and *England*; yet at *Upsal* they could observe nothing, but the whole sky beset thick with clouds, of a colour resembling that of the moon in a total eclipse, and variously agitated as by a wind, but this chiefly towards the south; which continued till 9 o'clock at night, a little after which it grew quite cloudy.

Elephants Teeth *and* Bones *found under Ground*; *by Sir* Hans Sloane. Phil. Trans. N° 403. p. 457.

IT is observable, that among the vast variety of extraneous substances, lodged, and found in several layers of the earth, at considerable depths, where it is impossible that they should have been bred, there are not so many productions of the earth, as of the sea: And again, among those which must have originally belonged to the earth, there are many more remains of vegetables than of land animals. It appears, however by the histories of past times, and the accounts of many, both ancient and modern authors, that bones, teeth, nay somtimes almost entire human and animal skeletons have been dug up in all ages of which we have histories, and almost in all parts of the world, whereof those, which were the most remarkable for their unusual size, have also been most taken notice of. Thus for instance, they have discovered in *Ireland*, the horns, bones, and almost entire skeletons of a very large sort of deer, which is commonly believed to have been the mouse-deer, an animal of an uncommon size; some of which kind are thought to be still alive in some remote and unfrequented parts of the continent of *America*.

Sir *Hans Sloane* confines himself chiefly to the elephant, and such bones, *dentes exerti*, or tusks, and teeth of this animal, as are either in his own possession, or have been mentioned by authors he has met with, to have been found under ground: And 1. As to those fossile teeth in his own collection, which undoubtedly did once belong to elephants, he adduces the following.

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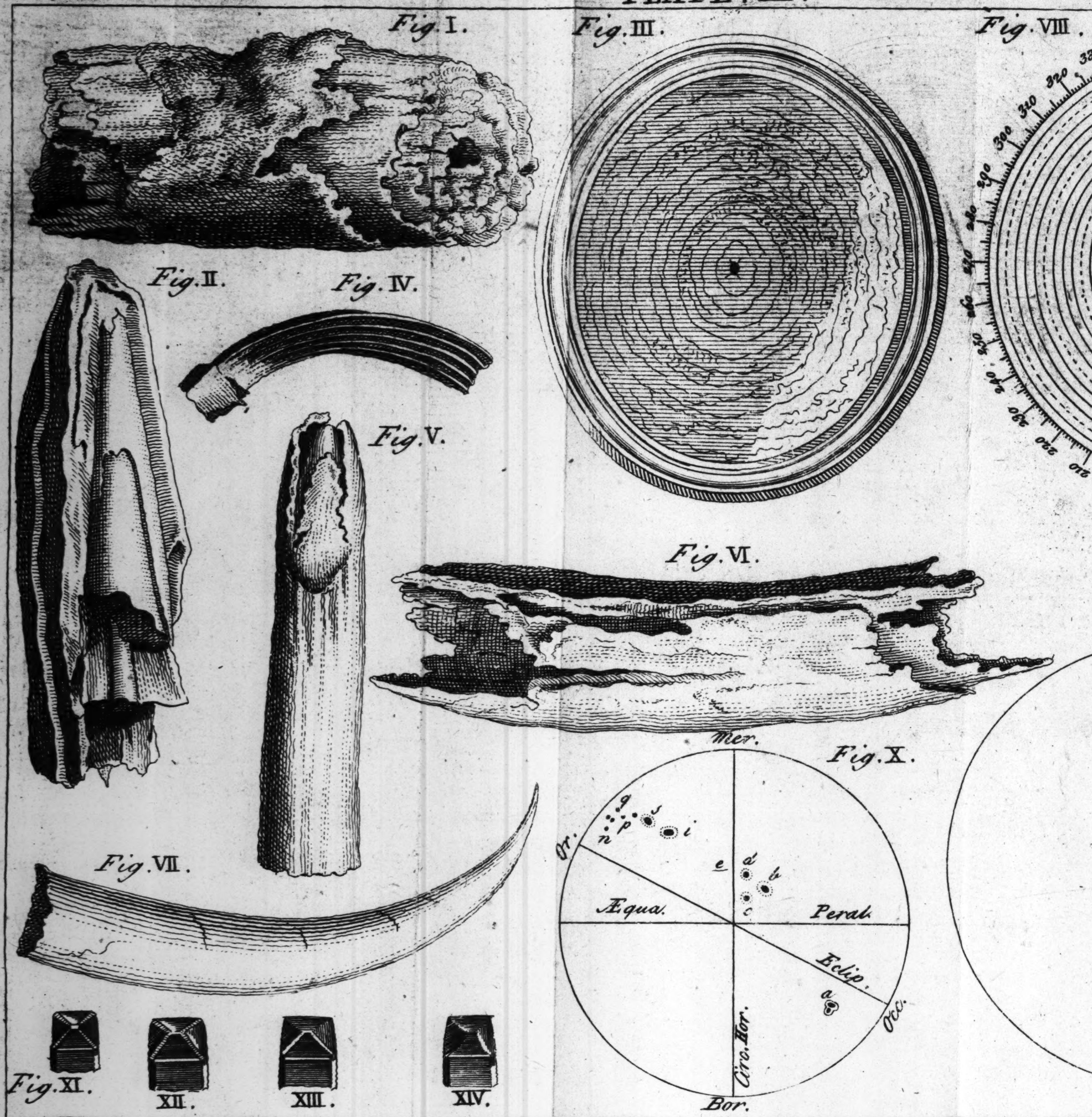
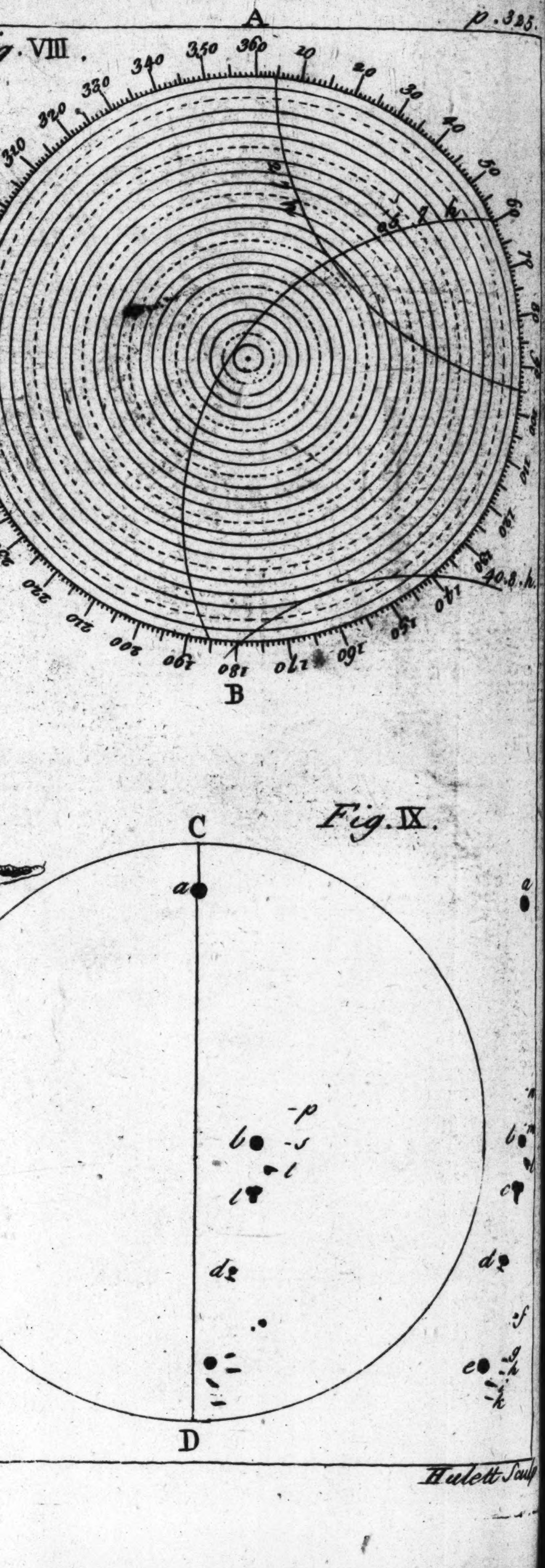
PLATE. IX.



G. Smith del.



N° 116. of his catalogue of quadrupeds and their parts, is he *dens exertus* or tuſk of an elephant, which was dug up 12 foot deep, from among ſand, or loom, as they were digging for gravel at the end of *Grays-Inn-Lane*, near *London*, and preſerved with tying it about with whale-bones and tape to keep it from falling to pieces by Mr. *Conyers*.

As the greateſt part of this tooth was fallen to pieces, nothing could be determined, as to its length, when entire: The largeſt piece, and alſo the moſt entire is 5 inches .8 in length, and 9 inches .6 in circumference; conſequently, ſomething more than 3 inches in diameter: This piece belonged to the baſis or bottom of the tooth, he means, that part by which it was articulated with the head; as appears by a cavity in form of a cone, which all theſe tuſks have at bottom; and which was filled in this with the ſand of the gravel-pit wherein it was found.

The condition this tooth was found in ſuggeſts the two following remarks. 1. It ſhews how far the ſubterraneous ſteams are apt to calcine ſubſtances of this kind, which was done in this tooth to ſuch a degree, that it was become exceeding brittle, and ready to fall to pieces; and had moreover acquired an aſtringent quality, common to calcined ſubſtances of this kind, which makes them ſtick pretty cloſe, when held to the tongue. They had altogether the ſame effect on the very large ſkeleton, found near *Drapani* in *Sicily*, and mentioned by *Boccatius*; and on that remarkable one found near *Tonna*, which has been deſcribed by *Tentzelius*; as alſo on two teeth found in *Northamptonſhire*, which Sir *Hans* ſhall next conſider: However, it does by no means follow from thence, that all teeth, and ſubſtances of this kind undergo the like calcination by lying long under ground; foraſmuch as there are others, as thoſe found in *Iceland*, and ſent to *Thomas Bartholin*, which were turned to a perfect hard, flinty ſubſtance.

It ſerves in the ſecond place to aſcertain the ſtructure of theſe teeth; and conſequently, of ivory in general, to be layer upon layer, or coat upon coat, like the ſkins in an onion, or rather the annual circles, or rings in trunks of trees. That this tooth is composed of different coats, ſurrounding and placed upon each other is very apparent by the largeſt piece remaining, repreſented by Fig. 1. Plate IX.

Sir *Hans* has already obſerved, that this piece belonged to the baſis of the tooth, and there appear in it very viſible marks of 9 coats, ſome of which are one tenth of an inch thick.

Towards

Towards the farther extremity of the tooth, where it tapers almost into a point, these several coats also unite into two or three, and those considerably thick, as represented by Fig. 2.

With some care these coats might be farther subdivided into a considerable number of other smaller ones, perhaps no thicker than a common parchment. Farther, the very manner of its falling to pieces is an evident proof of its structure, all the fragments being concave within, and convex without; and the lines of convexity, and concavity being fragments of concentric circles, which the several coats composed, when entire.

Thomas Bartholin in his treatise, entitled *de unicornu observationes novæ* p. 102. takes notice, that part of a fossil unicorn's horn having been calcined by order of *Christian* IV. king of *Denmark*, it was found to be composed, after the same manner, of thin layers upon layers: Whence he infers, that it was not the horn of an animal, as was commonly pretended, but a tooth, and that of a sort of whale in the northern seas, call'd *Narvhal*, as he afterwards had an excellent opportunity to verify by one of these unicorn's horns still sticking in the scull of the creature, which was sent to *Wormius* by *Thorlacus Scutonius*, Bishop of *Iceland*: Nor is this structure by any means to be looked upon, as an effect of the calcination, whether brought about by subterraneous steams, or by chemical process, but is natural to the tooth, as appears in some measure by a piece of ivory, marked 1181, and represented Fig. 3. which is still more plain in another marked 731, where several of these coats are by some disease in the tooth actually separated from each other, like the leaves of a parchment book, the ivory on the other side being still firm and close, as represented Fig. 4. This structure does likewise appear from the teeth of the very young elephant which died at *London*; where the uppermost coat being very moist crack'd upon drying, and broke at the top, as represented Fig. 5.

N° 750. is part of another tusk, which Sir *Hans Sloane* had from Mr. *Morton*, who in his *Natural History* of *Northamptonshire*, p. 252. gives the following account of it: 'An extraordinary elephant's tooth, namely one of those which grow 'out of the upper jaw; and which for their magnitude and 'length have by some authors been accounted horns was lately 'dug up in *Bowdon-parva* field: Even its native colour hath 'been in a great measure preserved; but it is become brittle by 'lying in the earth; and was broken into 3 or 4 pieces trans'versely in digging it up: The two larger pieces of it were

'presented

presented to me; one of them is somewhat above a yard; the other is two foot in length; but the whole tooth must needs have been at least 6 foot long — the thickest part of the biggest piece in my possession is 16 inches round: The tooth lay buried more than 5 foot deep in the earth. The *strata* from the surface downwards to the place where the tooth was lodged, were as follows. 1. Soil 13 or 14 inches. 2. Loam 1 foot and ½. 3. Large pebbles, with a small mixture of earth among them 2 foot and ½. 4. Blue clay; in the upper part of this *stratum* the tooth was found.' Thus far Mr. *Morton*. That part of this tooth, which is in Sir *Hans Sloane*'s custody bears again very visible marks both of the calcination it underwent by lying in the earth, and of its laminated structure, as represented Fig. 6.

N° 1185 (Fi. 7.) is the tusk of an elephant, remarkable for its large size, and for its being so very entire. It was found under ground in *Siberia*. It is of a brownish colour, and hollow at bottom like other elephants teeth, one of which it plainly appears to be. From the basis, measuring along the outer circumference to the small end, it is 5 foot, 7 inches long, and along the inner circumference 4 foot, 10 inches; measuring from the inside of the basis to the small end in a streight line the distance is 3 foot, 10 inches and ½; at the basis, where thickest, it measures 1 foot, 6 inches round; and is there 6 inches in diameter; it weighs 42 pounds: The like tusks and other bones of the same animal, namely the elephant, are found in sundry parts of *Siberia* in considerable quantities; and the tusks and teeth in particular, when less corrupted, are used all over *Russia* for ivory. *Henricus Wilhelmus Ludolfus*, in the appendix to his *Russian* grammar p. 92, mentions them among the minerals of *Russia*, by the name of *Mammotovoikost*, and takes notice, that the *Russians* believe them to be the teeth, and bones of an animal, living under ground, larger than any of those above ground. They use it in physic in lieu of, and for the same purposes with the unicorn's horn; and *Ludolfus* himself having been presented with a piece by one of his friends, who said, he had it from a *Russian* of great quality, lately returned from *Siberia*, found it to be true ivory: He adds, that the most sensible among the *Russians* affirm them to be elephants teeth, brought thither at the time of the deluge. The description of these teeth and bones given by *E. Ysbrants Ides* in his travels from *Moscow* to *China*, is still more extensive, and withal so particular, that his whole passage deserves to be transcribed at length.

length. 'Amongst the hills *says he*, which are situated to the 'north-east of *Makofskoi*, not far from thence the *Mummut's* 'tongues and legs are found; as also in particular on the banks 'of the rivers *Jenize*, *Trugan*, *Mongamsea*, *Lena*, and near '*Jakutskoi*, to as far as the frozen sea. In the spring, when 'the ice of this river breaks, it is driven in such vast quan-'tities, and with such force by the high swoln waters, that it 'frequently carries very high banks before it, and breaks off 'the tops of hills which falling down, discover these animals 'entire, or only their teeth, almost frozen to the earth, which 'thaw by degrees. I had a person with me to *China* who an-'nually went out in search of these bones: He told me, as a 'certain truth, that he and his companions found a head of one 'of these animals, which was discovered by the fall of such a 'frozen piece of earth. As soon as he opened it, he found 'the greatest part of the flesh rotten; but it was not without 'difficulty, that they broke out his teeth, which were placed 'before his mouth, as those of the elephant are; they likewise 'took some bones out of his head, and afterwards came to his 'fore-foot, which they cut off, and carried part of it to the city 'of *Trugan*, the circumference of it being as large, as that of 'the waist of an ordinary man: The bones of the head ap-'peared somewhat red, as tho' they were tinctured with blood. 'There are different reports concerning this animal. The 'heathens of *Jakuti*, *Tungusi* and *Ostiacki*, say, that they 'continually, or at least by reason of the very hard frosts, 'mostly live under ground, where they go backwards, and for-'wards; to confirm which they tell us, that they have often 'seen the earth heav'd up, when one of these beasts was on the 'march; and after he was past the place, sink in, and thereby 'make a deep pit: They farther believe, that if this animal 'come so near the surface of the frozen earth, as to smell or 'discern the air, he immediately dies, which they say is the 'reason that several of them are found dead on the high banks 'of the river, where they unawares come out of the ground: 'This is the opinion of the infidels concerning these beasts, 'which are never seen. But the old *Siberian Russians* affirm, 'that the *Mammuth* is very like the elephant, only with this 'difference, that the teeth of the former are firmer, and not so 'streight as those of the latter: They are likewise of opinion 'that there were elephants in this country before the deluge, 'when this climate was warmer; and that their drowned

'bodies

'bodies floating on the surface of the water of that flood 'were at last wash'd and forced into subterraneous cavities; 'but that after this *Noachian* deluge, the air, which before 'was warm, was changed to cold; and that these bones have 'lain frozen in the earth ever since; and so are preserv'd 'from putrifaction, till they thaw and come to light, which 'is no very unreasonable conjecture; tho' it be not absolutely 'necessary, that this climate should have been warmer before 'the flood, since the carcasses of drowned elephants were very 'likely to float from other places, several hundred miles 'distant to this country, in the great deluge which covered 'the surface of the whole earth. Some of these teeth, which 'doubtless have lain the whole summer upon the shore, are 'entirely black and broken, and can never be restor'd to 'their former condition; but those which are found in good 'case are as good as ivory; and are accordingly transported 'to all parts of *Muscovy*. The abovementioned person like'wise told me, that he once found two teeth in one head, 'that weigh'd upwards of 12 *Russian* pounds, which amounts 'to 400 *German* pounds: So that these animals must neces'sarily be very large, tho' a great many lesser teeth be found. 'By all that I could gather from the heathens, there is no 'person ever saw one of these beasts alive, or can give any 'account of its shape.'

Thus far *E. Ysbrants Ides.* What he observes of these teeth that are black and broken, may serve as a comment to the following passage of *Pliny lib.* 36. *c.* 18. '*Theophrastus* 'relates, that there is found a fossile ivory of a white and 'black colour, and that bones grow out of the earth, and that 'there are likewise found bony stones.'

Lawrence Lang, in the *Journal of his Travels to China*, (vide *Present State of Russia* vol. 2. p. 14) whither he went with dispatches from his *Czarish* Majesty in 1715, takes notice of these bones, as being found about the river *Jenisei*, and towards *Mangasea*, along the banks, and in the pits occasioned by the fall of the earth: He calls them *maman-bones*, and informs us, that some of the inhabitants are of opinion, that they are no real bones, teeth, &c. but a sort of fossile horn, that grows in the earth; and that others will have them to be the bones of the *Behemoth*, mentioned in the 40. chap. of *Job*; the description whereof they pretend fits the nature of the beast, whose bones and teeth they are imagined to be; those words in particular, *viz.* that he *is*

caught with his own eyes, agreeing with the *Siberian* tradition, that the *maman* beast dies upon coming to light.

The same author affirms, from the report, as he says, of credible people, that sometimes there have been found horns, jaw-bones and ribs, with fresh flesh and blood sticking to them. The same is confirmed by *John Bernard Muller*, in his account of the *Ostiacs ibid.* p. 52. who adds, that the horns in particular have been found sometimes, 'all bloody at the 'broken end, which is generally hollow, and fill'd with a 'matter resembling concreted blood; that they find, together with these teeth or horns, as he calls them, the scull and jaw-bones with the grinders still sticking in them, all of a monstrous size; and that he himself, with some of his friends had seen a grinder weighing upwards of 24 pounds; that the inhabitants make divers sorts of work of these teeth; and that they are mostly to be met wih in the coldest parts of *Siberia*; as for instance *Jackutsky*, *Beresowa*, *Mangasea*, and *Obder*: He likewise gives the description of one of these animals from the accounts of several persons, who assur'd him, that they had observ'd them in the caverns of the high mountains beyond *Beresowa*: But as this description has very much the air of a fable, Sir *Hans* forbears inserting it. The author of the *Present State of Russia* Vol. I. p. 12. observes, that some of the *Swedish* prisoners banish'd into *Siberia*, got their livelihood by turning snuff-boxes out of these teeth; and in another place, p. 78. he mentions them among the *Siberian* commodities, of which the *Czar* has the monopoly.

The accounts Sir *Hans Sloane* has hitherto given of these *maman* bones and teeth, or at least their most essential parts, are confirmed by a letter of *Basilius Tatischow*, director general of the mines in *Siberia*, and counsellor of the *Czar's* metallic council, wrote to the learned *Ericus Benzelius*, Bishop of *Gothenburg*, and published in the *Acta Liter. Sueciæ*. 1725. *Trimestre secundum*, p. 36. wherein he mentions the following pieces he had in his own possession: A large horn, as he calls it, or tooth, weighing 183 pounds, which he made a present of to his *Czarish* Majesty, and is kept in the *Czar's* collection of curiosities at *Petersburg*; and another large horn, which he presented to the Imperial Academy at *Petersburg*; another still larger than either of these two, which he caus'd to be cut, and he himself carv'd several things of it, the ivory being very good; part of the

scull

ſcull; corrupted by having lain in the ground, and ſo large, that it ſeemed to him to be of the ſame ſize with the ſcull of a large elephant; the forehead in particular was very thick, and had an excreſcence on each ſide, where the horns uſually ſtick to it; which excreſcence, however, as the author obſerves, was ſo ſmall, as to make him doubt, whether or no there were ever any horns ſtuck to them. The cavity, in which the brain was lodged, was exceeding ſmall in proportion to the bulk of the ſcull: He alſo found a ſpongy bone a foot and a half long, and three inches broad, ſticking to the ſcull, and of a conical figure: Whence he conjectured, that it ſerv'd to ſupport one of the horns, which is likewiſe obſerv'd in other animals that bear horns: Laſtly, a grinder 10 inches long and ſix inches broad, beſides ſeveral of the ribs, ſhank-bones, and other bones found from time to time, which the author forbore mentioning.

The ſame author hath taken no ſmall pains to enquire into the true ſtate of thoſe pits and hollows, which the *Pagan* inhabitants of *Siberia* ſay, theſe animals make when they walk under ground, and found that they were nothing but caverns, ſuch as are common in other mountainous countries, and are owing to the force of ſubterraneous rivers and cataracts, which at laſt eat thro' and undermine the places where they paſs; ſo as to make the ground above them give way, and ſink in. This is what Sir *Hans Sloane* found remarkable in this letter of M. *Tatiſchow*.

Sir *Hans Sloane* cannot forbear adding, that tho' the author hath left the grand queſtion about the origin of theſe bones undetermined; yet his obſervations ſeem to him to contribute very much to eſtabliſh the opinion above related, that theſe bones are the bones, and the horns, as he calls them, the tuſks of elephants, drowned in the univerſal deluge. It is to be hoped that this matter will one time or other be ſet into a ſtill clearer light, particularly after the order his late *Czariſh* Majeſty gave the Governor General of *Siberia*, to ſpare no care or coſt to find an entire ſkeleton of this animal, and ſend it to *Tatiſchow*.

Before Sir *Hans Sloane* proceeds farther, he adds one obſervation of *Cornelius le Brun*, who in his travels thro' *Ruſſia* to the *Eaſt Indies*, tells us, that in the neighbourhood of *Veronitz*, they had found ſeveral elephants teeth on the ſurface of the ground, which no body could tell how they came thither, and that the *Czar*'s opinion about them was,

 that

that *Alexander the Great*, when he paſſed the *Tanais*, or *Don*, advanced as far as *Koſtinka*, a ſmall town eight werſts from thence; and that probably ſome of his elephants died there, of which thoſe teeth were the remains.

N° 764 of Sir *Hans Sloane*'s collection is one of the grinders of an elephant, which was likewiſe found in *Northamptonſhire*, the deſcription of which is thus in Mr. *Morton*'s words, *Natural Hiſtory of Northamptonſhire*; c. III. §. cxxxv. p. 252. ' Northwards, ſays he, about 50 yards from this ' place (where the abovementioned *dens exertus* was found) ' was alſo dug up one of the *Molares* of an elephant, ' perhaps of that the tuſk belonged to. It exhibited 13 or ' 14 parallel *lamellæ*; each of which extended the whole ' length, and almoſt the whole thickneſs of the tooth, and of ' theſe it is moſtly compoſed. But in a live or perfect tooth, ' theſe *lamellæ* do not appear ſo plainly, being in part cruſted ' over with a white oſſeous ſubſtance, which in this foſſile ' tooth was almoſt wholly periſhed and gone, inſomuch that ' the *lamellæ* were more expoſed to view: From the root to ' the top in the longeſt part, which was alſo near its middle, ' it was juſt ſeven inches long: Its thickneſs in the thickeſt ' part of the root, which was alſo near the middle, was ' almoſt three inches, and it was a little more than eight ' inches broad. Meaſuring it this way, we take in the whole ' pile of the *lamellæ*: None of the *lamellæ* were contiguous; ' between them interpoſed a thinner plate of a whiter colour, ' and a laxer texture: Three or four of the outmoſt, at one ' end of the pile, appear undulated at the top of the tooth, ' and are near as broad at top as at the root, and have a ' blunt termination: The reſt of them are by degrees con' tracted to a point, and are gradually ſhorter to the other ' extremity of the pile, and alſo bend a little over one ' another: And each of them, as they approach the top, ' divides, as it were, into ſeveral ſmaller teeth; and with ' theſe the *lamellæ* of this figure terminate. The above' deſcribed tooth was lodged at almoſt 12 foot depth in the ' earth: Above it were the following *ſtrata*. 1. The top ' earth, a blackiſh clayey ſoil, about 16 inches. 2. Sandy ' clay intermixed with pebbles, five foot. 3. A blackiſh ſand ' with ſmall white ſtones in it, one foot. 4. A loamy ſofter ' ſort of gravel, one foot. 5. A ſharper gravel, about two ' foot. The tooth was found a foot and a half deep in this ' *ſtratum* of gravel. Below this fifth *ſtratum* was a blue ' clay.'

'clay.' Thus far Mr. *Morton*. It is very viſible that this grinder alſo, by lying in the earth, has undergone the ſame alteration with the tuſk above deſcribed, found in *Bowden-parva* field.

N° 119 and 120 of Sir *Hans Sloane*'s collection are two pieces of another large grinder, very probably of an elephant too, turned to a very hard, ſtony, and almoſt metallic ſubſtance.

N° 121 is a piece of the *molaris*, or grinder of an elephant, where the undulated *lamellæ* are ſet very cloſe to each other.

N° 122 is a piece of another grinder, probably of an elephant; it hath very apparent marks of being foſſile, as well as the preceeding; and it is farther remarkable, that a petrifying ſubſtance being got between the *lamellæ* hath very conſiderably ſeparated, and divided them from each other, in ſuch a manner, that they appear to have been ſet very looſe.

N° 427 of Sir *Hans Sloane*'s collection of quadrupeds and their parts, is a portion of an elephant's ſcull, which was found at *Glouceſter* after the year 1630, together with ſome large teeth, ſome five, others ſeven inches in compaſs, according to a ſhort inſcription written upon this very piece.

A Solar Eclipſe *and other* Aſtronomical Obſervations *near* Lisbon; *by* F. Carbone. Phil. Tranſ. N° 403. p. 471. *Tranſlated from the* Latin.

SEPT. 15. 1727. N. S. *mane* F. *Carbone* with a teleſcope of about eight foot long, and fitted with a micrometer, obſerv'd this eclipſe at a farm more weſterly than St. *Anthony*'s College by 4″ of time, and whoſe Lat. he found by an aſtronomical quadrant of three foot to be 38° 42′ 52″. The beginning of the eclipſe happened below the horizon; and now about four digits of the ſun were eclipſed, when he firſt appeared from behind an oppoſite hill: The following phaſes were the only ones he could obſerve.

Digits	Immerſion	true time corr. H.	M.	S.	
VI and a half	——— ———	5	55	8	doubtful
VIII	——— ———	6	10	54	doubtful
VIII 1′ 48″ greateſt obſcuration	———	6	13	29	nearly

Digits

Digits	Emersion	true time corr. H.	M.	S.
VI and a half		6	31	49
VI		6	35	23
V and a half		6	38	45
V		6	41	57
IV and a half		6	45	2
IV		6	47	59
III and a half		6	50	49
III		6	53	34
II and a half		6	56	16
II		6	58	54
I and a half		7	1	28
I		7	3	59
Half		7	6	28
End of the eclipse		7	9	2 most cert

Immediately after the end of the eclipse, F. *Carbone* examined his pendulum-clock by two altitudes of the sun taken with the same astronomical quadrant; and this correction he applied in the above-mentioned phases.

Oct. 15. With a 22 foot telescope F. *Carbone* observ'd an immersion of *Jupiter*'s innermost satellite, which happened 9h. 10′ 54″.

Nov. 7. With the same telescope he observ'd the immersion of the said satellite 9h. 25′ 45″.

The following observations were made at *Rome* at the foot of the *Quirinal* hill, on a solar eclipse in the morning of the same day, namely *Sept.* 15. 1727. The hours, minutes, and seconds of true time after midnight, are rectified by a transit of the sun.

h.	′	″	
7	0	0	The author was intent upon observing *Sirius* near the meridian; when according to the ephemerides the beginning of the eclipse was to happen some few minutes after: In the mean time directing his telescope towards the sun, and receiving his image on paper, he observ'd that the eclipse was begun some minutes before. The preceeding day he observ'd several *maculæ* appear on the sun's disk, which being desirous to represent in their proper places in Fig. 9. Plate

h.	'	"	
			Plate IX. before the beginning of the eclipse, he directly applied himself to delineate them; after which he accurately noted the following phases together with S. *Maraldi*.
	2	17	Three digits and a quarter of the sun's disk are covered by the moon. The intersection of the disks is in 5° and 95°, reckoned from the vertical point, A, in the inverted Fig. 8. the *macula a* is in the plane of the azimuth, drawn from the zenith thro' the sun's center, and is distant in the semi-diameter of the sun's disk from his center towards the periphery 4 digits and $\frac{3}{4}$, as represented in Fig. 9. and the *macula e* is likewise in the same azimuth nearly.
7	21	47	Clouds for some time intercepting the sight of the sun, and it clearing up a little before, now 4 digits and a half of the sun's disk are covered by the moon. The intersection of the disks is in 10° and 111° of the solar disk, reckoned as before, from the point A of the sun's image towards the left.
7	24	0	The more dilute *macula m* near *b* enters the moon's disk.
7	24	40	The *macula b* begins to be touched by the moon's disk.
7	25	11	*b* is entirely immerged.
7	27	41	The beginning of the *macula c* begins to enter the moon's disk.
7	28	31	*c* is entirely hid.
7	29	10	Five digits and a half of the sun's diameter hid.
7	31	9	Five digits and $\frac{3}{4}$ hid. The intersection of the disks is in 20° and 136°.
7	38	45	Six digits hid; 31° and 150° are the points of intersection of the disks.
7	40	58	The former limb of the *macula d* is touched by the moon.
7	41	45	*d* is now entirely covered.
7	43	15	Six dig. and $\frac{1}{4}$ of the sun are covered; and the intersection of the disks is in 39° and 162°.

h. ' "

h.	′	″	
7	45	26	The limb of the *macula f* which almost disappears, is touched by the moon.
7	46	20	*f* is entirely covered by the moon.
7	50	0	Six dig. and ¼ covered, and the peripheries of both luminaries intersect each other in 61° and 185°.
8	0	12	A little less than 6 dig. or 5 ⅞ are hid; and the disks intersect each other in 62° and 182°.
8	2	25	Five dig. and ¾ are covered; and the disks intersect each other in 63° and 183°.
8	5	24	Five dig. and ¼ are hid; the luminaries intersect each other in 80° and 192° of the sun's disk.
8	8	32	Five dig. eclipsed; the luminaries intersect each other in 82° and ½ and in 192° and ½.
8	11	50	The former limb of the *macula b* begins to emerge.
8	12	38	*b* entirely emerged out of the moon's disk; and the *macula n*, next to it, likewise emerges.
8	14	46	The *macula m* next to *b* likewise emerges.
8	16	34	The *macula l* next to *b* likewise emerges.
8	18	29	The *macula c* emerges.
8	22	38	Three dig. and ½ eclipsed; the intersection of the disks is in 105° and 195°.
8	23	40	The *macula d* begins to emerge.
8	24	10	*d* entirely emerged.
8	27	23	Two dig. and ½ eclipsed; the intersection of the disks in 115° and 182°.
8	34	5	The former limb of the *macula e* begins to emerge.
8	34	55	*e* entirely emerged out of the moon's limb.
8	35	46	One dig. and ½ eclipsed.
8	37	9	One dig. and ¼ eclipsed.
8	37	27	The *maculæ g* and *h*, situated near the *macula e*, emerge out of the moon's limb.
8	39	46	¾ of a dig. eclipsed; the intersection of the limbs of both luminaries is in 140° and 180°.
8	42	8	The superior limb of the sun is distant from the vertex 58° 1′, viewed thro' a brass quadrant of three *Roman* palms radius, fitted with a telescope.

8 44 10

h.	′	″	
8	44	10	The end of the eclipse; when the extremity of the moon's disk entirely emerges out of the sun's limb.
8	46	53	The superior limb of the sun, viewed thro' a brass quadrant is distant from the vertex 57° 30′; consequently the sun's center is distant from the vertex 57° 46′.
8	48	1	The superior limb of the sun, viewed again, is distant from the vertex 57° 20′; consequently, the sun's center is distant from the vertex 57° 36′.
			The sun's inferior limb distant from the vertex 57° 20′; consequently the sun's center is distant therefrom 57° 4′.
			The same day at noon the sky very clear.
11	58	25	The first limb of the sun with the *penumbra* touches the meridian line.
0	1	35	The second limb with the *penumbra* touches the meridian line; therefore noon was 0° 0′ 0″.
			At noon the distance from the vertex was found by a quadrant.
			The superior limb 38° 27′ } The sun's center 38° 43′.
			The inferior limb 38 59 }

AB (Fig. 8. Plate IX.) represents the plane of a vertical circle, drawn thro' the sun's center. The sun's disk is divided into 12 dig. and $\frac{1}{4}$ of a digit; where the degrees of the periphery (360) are marked, reckoning them from the *apex* A to the right hand; that in each phasis the common intersection with the moon's disk may be indicated.

The *maculæ* CD, in (Fig. 9.) the sun's disk delineated in their proper places are here repeated to shew by letters the indication of each in the series of the observations CD; represents the plane of a vertical circle drawn thro' the sun's center.

A solar Eclipse *observed the* 15. *of* Sept. 1727. N. S. *in the Observatory of* Bononia; *by* S. Manfredi. Phil. Trans. N° 403. p. 477.

AT the time of this eclipse a great many *maculæ* were observed in the sun; but as, by reason of clouds, S. *Manfredi* could not make the necessary observations about them, to find their situation on the sun's disk, the principal *maculæ* only are

are exhibited in Fig. 10 Plate IX. as their places could be inferred from obſervations made on the evening of 14. *Sept.* about 5^h 20′; and he takes that to be ſufficient to ſhew ſuch as were covered by the moon at the time of the eclipſe: Yet it is to be noted, that each of the ſaid *maculæ*, from the time they were obſerved, namely on the evening of the 14. till the time of the eclipſe, did not only proceed in its own ſolar parallel, as far as the regular motion of the ſun would permit, but likewiſe that moſt of them appeared irregularly changed both in figure and magnitude; and that ſome of them were alſo divided into ſeveral *maculæ*, and others again united into one; and laſtly that ſome were obſerved in the eaſtern limb of the ſun, which did not appear the preceeding evening.

The beginning of the eclipſe could not be obſerved; but at 18^h 55′ 48″ aſtronom. the eclipſe appeared as ſenſible between clouds.

h.	′	″	
18	59	37	One digit and probably more, was eclipſed.
19	3	12	A dig. and $\frac{1}{2}$.
19	6	50	Two dig.
19	10	11	Two dig. and a half; doubtful. A little after, clouds cover the ſun.
19	30	35	Between the clouds the ſun ſeems to be eclipſed upwards of four dig. and a half.
19	35	46	The eclipſe does not yet ſeem to reach to 5 dig.
19	40	47	About five dig.
19	44	31	The *maculæ b*, *c*, *d*, *e* were now hid, as alſo the center of *i*.
19	47	27	The moon's limb between the *maculæ s*.
19	47	46	The ſecond of the two *maculæ s* entirely cover'd.
19	51	37	The eclipſe is a little leſs than five digits; between clouds.
19	54	12	The centre of the *macula p* covered.
19	55	37	The centre of *q* is likewiſe covered.
19	59	2	Four dig. and a half eclipſed; doubtful.
20	1	22	One of the *maculæ* near the limb (probably *n*) is covered.
20	10	10	The eclipſe is a little leſs than 3 dig. and $\frac{1}{2}$.
20	11	47	The *macula i* entirely emerged.
20	15	0	Three dig. eclipſed.
20	18	48	Two dig. and a half.
20	18	49	The *macula s* begins to emerge.

h. ′

h.	′	″	
20	22	26	About two dig. eclipsed.
20	26	14	One dig. and a half.
20	36	6	The end of the eclipse taken notice of by three observers, agreeing in the very same second of time.

Fig. 10. Plate IX, represents the solar *maculæ* in an inverted position.

An Observation of a solar Eclipse *at* Padua Sep. 15. 1727. N. S; *by* S. Polenus. Phil. Transf. N° 403. p. 479. *Translated from the* Latin.

THICK clouds frequently intercepting, no more than the following phases could be observed.

True time.				Dig.	
h.	′	″			′
19	3	45	— — — —	0	10
	24	12	— — — —	3	9
	41	27	— — —	4	30
20	30	45	— — —	1	30
	38	42	The end of the eclipse.		

Some farther Observations towards composing a Natural History of Mines *and* Minerals; *by Dr.* Nicholls. Phil. Transf. N° 403. p. 480.

IN a former *Transaction* Dr. *Nicholls* gave some particulars relating to mines in general; and that the *loads* in *Cornwall* yielded iron, tin, lead and copper.

Of all the substances concurring to form the terrestrial globe, iron probably bears the greatest share; as it not only abounds in most kinds of stone, shewing itself in varieties of *crocus*, all which gain a more intense colour by fire; but likewise enters greatly into the composition of common clay, as may be judged from the similitude of colour between clay and dry iron ore; from the easy vitrification of clay; from the resemblance between clay so vitrified, and the clinkers of iron; from its deep red colour after calcination; and lastly from its yielding pure iron, by being burned with oil.

But while iron is thus entangled with other bodies, it rarely employs the care of the miner, who finds the expence of reducing

ducing it to metal too ſeldom balanced by the price it yields: For which reaſon, tho' we frequently meet with large and rich *loads* of iron; yet (the woods having been applied to more advantageous uſes) they are there entirely neglected.

When it is moſt pure, the Dr. finds the ore under three different appearances.

1. A rich dry iron ore, whoſe ſcrapings exactly reſemble an *alkohol Martis*: This kind of iron ore has very nearly the colour of common clay.

2. A rich iron ore, with part of the wall of the load, form'd by a concretion of yellow cryſtals: In this ſtone the iron radiates from points forming ſegments of ſpheres; and where theſe ſpheres leave any interſtices, you find a *crocus* or oker.

3. A ſtone of iron of the kind, made uſe of for burniſhing plate; it is of the ſpecies of the *hæmatites*.

Both theſe laſt ſtones ſcrape into a deep *crocus*.

From the ſecond inſtance we may conjecture, that the yellow colour in cryſtals ariſes from a *crocus* entangled with the ſtony ſalts.

Tho' the want of wood in *Cornwall* deprives it of the advantages it might otherwiſe reap from iron as a metal, we ſhall nevertheleſs find it far from being an uſeleſs metal; when we conſider it as ſometimes impregnating the waters with vitriolic ſalts, thereby making them a proper *menſtruum* for diſſolving the diſſeminated particles of metals; ſometimes deſtroying the ſulphureous *menſtrua*, which (tho' they diſſolve the diſſeminated metals) do nevertheleſs obſtruct their new concretions; and ſometimes as being itſelf the magnet by which the metallic particles are attracted into new concretions.

The next metallic ſubſtance found in *Cornwall*, and from which theſe iſlands are ſuppoſed to take their name, is tin: It is never found but as an ore; whereas gold is never found but as a metal, at leaſt its ore is unknown; and all other metals are found ſometimes as a metal, and ſometimes as an ore.

Tin always ſhoots into cryſtals, which are of different magnitudes from two ounces in a ſingle cryſtal, down to ſuch as eſcape our ſight: Theſe cryſtals are for the moſt part interſperſed in loads of other ſubſtances. As,

1. Tin cryſtals interſperſed in a load of a kind of clay, in which is obſervable a conſiderable quantity of red-okre.

2. A ſtone of hard iron ſtone, in which are exceeding ſmall cryſtals of tin.

3. Some-

3. Somewhat larger cryſtals, interſperſed in a dry red-okre.

4. Tin cryſtals, interſperſed with ſpar ſtone, and with a ſort of marle.

5. Larger cryſtals, interſperſed in a kind of clay and red okre as in N° 1.

When a hundred ſacks of the load (each containing more than a *Wincheſter* buſhel) yield one gallon of clean ore, the load is eſteemed very well worth working.

Sometimes theſe cryſtals are collected into one maſs in ſuch a manner, as to form loads of pure tin ore, and ſo large, as to yield to the value of 100 *l.* every 24 hours; as

1. Stones of ſuch pure loads, of which the one is black, and the other nearly white.

Theſe cryſtals ſometimes concrete into the form of a parallelipepidon, whoſe ſummit is covered by a pyramid; ſometimes the angles, formed by the ſides of the pyramid, and ſometimes the ſummit of the pyramid, are plained away, as it were. As,

A whole cryſtal, which has none of its angles off, as repreſented Fig. 11. Plate IX. A cryſtal, which has only two of its angles plained away, as repreſented Fig. 12. A cryſtal, which has all its angles plained away (Fig. 13.) A cryſtal, which has all its angles, and its ſummit plained away (Fig. 14.)

Sometimes the cryſtals repreſent two equal pentelateral pyramids, joined at their baſe.

Under whatſoever form theſe cryſtals ſhoot, they always carry an exceeding fine ſurface; which, when rubbed off, can be renewed by no art. In Fig. 14. one ſide of the parallelipepidon is rubbed away, to ſhew how it appears after loſing its natural ſurface.

Theſe cryſtals are of different colours, from the white (reſembling white ſugar candy) to the deep black. The white cryſtals ſeem to Dr. *Nicholls* to carry a finer luſtre than any other he ever ſaw, and are perfectly tranſparent: So that were they found of equal ſize with the black cryſtals, and of a white water (which he imagines may happen) their hardneſs and weight (in both which they exceed any other foſſil) would probably make them preferable to the diamond: However, as deeper colours of theſe cryſtals ſeem to ariſe from a greater proportion of iron in their compoſition, which they throw off in an iron ſlag upon fuſion, and which changes by proper degrees of heat into a *crocus*, thereby changing the colour of the cryſtal to a bright red; ſo the white tin ore is certainly to be eſteemed both richeſt and beſt, as moſt free from iron.

There

There was a piece of the load, in which the cryſtals are o
a brighter red, from its being heated red hot.

Theſe cryſtals ſeem to be the heavieſt bodies the earth pro-duces, except quickſilver and actual metals: Their ſpecific gra-vity is to water, as 90 and $\frac{1}{2}$ to 10; to rock cryſtal in water as 90 and $\frac{1}{2}$ to 26; to diamond, as 90 and $\frac{1}{2}$ to 34; and to pure malleable tin, as found by repeated trials, as 90 and $\frac{1}{2}$ to 78: Whence appears the poſſibility of what ſome miners affirm *viz.* that a cubical inch of ſome tin ores will yield more than a cubical inch of metal.

Having already taken notice that the cryſtals of tin are ſome-times ſo ſmall, as to eſcape the eye, and ſo diſſeminated in the load, as not to make above $\frac{1}{800}$ or $\frac{1}{1000}$ part of the load, one would naturally imagine it an endleſs labour to cleanſe the ore from ſuch a vaſt diſproportion of rubbiſh: But the great ſpecific gravity of theſe cryſtals renders the cleaning it leſs troubleſome and leſs expenſive, than in any other ore whatever. It requires no more than that the whole ſtuff be ſtamped to a fine powder after which it is waſhed by a water, whoſe force is ſo mode-rated, as to waſh away only the lighteſt parts. This ſtamping and waſhing is repeated, till the ore be left exceeding clean and yield in metal from $\frac{18}{20}$ to $\frac{10}{20}$, according as it is cleanſed from the load, and as it is in its own nature more or leſs free from iron.

A Method of raiſing ſome exotic Seeds, *which have been judged almoſt impoſſible to be raiſed in* England; *by Mr* Philip Miller. Phil. Tranſ. N° 403. p. 485.

MR. *Miller* gives an account of the methods he had taken to raiſe the coco-nut, with the ſucceſs of each; which has led him to a ſure method for raiſing ſuch ſeeds, as have hard coats, or ſhells ſurrounding them; and have been judged very difficult, if not impoſſible to be raiſed in *England.*

In the year 1724 he had a parcel of freſh coco-nuts given him which were brought over from *Barbadoes:* From part of theſe nuts he ſtripped off their outer coat, or huſk, and the other part he left entire, as he received them. He planted both theſe parcels in large pots, filled with good freſh earth, and plunged the pots into a hot-bed, made with tanners bark; giving them gentle and frequent waterings, as the earth in the pots ſeemed to require; but had not one, out of the whole number, that
made

made any attempt to ſhoot, as he could perceive; and upon taking them out of the pots, he found them very rotten.

About four months after, he receiv'd another freſh parcel of coco-nuts from *Barbadoes*, which he treated in another manner; from part of theſe he cut off the outer coat or huſk, and the other part he left entire, as before: But ſuppoſing it was owing to his planting the other parcel in pots, that they did not ſucceed; he made a freſh hot-bed, with horſe-dung, and cover'd it over with freſh earth, about 18 inches thick, in which he planted the nuts; obſerving as before, to ſupply it with convenient moiſture, as alſo to keep the hot-bed in an equal temparament of heat (which he was guided to do by a thermometer, graduated for the uſe of hot-beds) but with all his care he had no better ſucceſs than before; not one of the nuts making any eſſay towards ſhooting.

The following year he had another parcel of coco-nuts given him, which, conſidering his former ill ſucceſs, he planted in a different manner, as follows. Having a hot bed which had been lately made with tanners bark, and which was fill'd with pots of exotic plants, he remov'd two of the largeſt pots, which were placed in the middle of the bed, and opening the tanners bark under the place where the two pots ſtood, he placed the two coco-nuts therein, laying them ſide-ways, to prevent the moiſture (which might deſcend from the pots) from entering the hole at the baſe of the fruit, and thereby rot the ſeminal plant upon its firſt germinating: He then cover'd the nuts over with the bark two or three inches thick, and placed the two pots over them in their former ſtation. In this place he let the nuts remain for ſix weeks; when removing the two pots, and uncovering the nuts, he found them both ſhot from the hole in the baſe of the fruit, an inch in length; and from the other end of the fruit ſeveral fibres were emitted two or three inches in length. Upon finding them in ſuch a forwardneſs, he took them out of the bark, and planted them in large pots, fill'd with good freſh earth, plunging the pots down to their rims in the tanners bark, and covering the ſurface of the earth in the pits half an inch thick with the ſame: Soon after, the young ſhoots were more than two inches long, and continued to thrive very well. Mr. *Miller* communicated this method to ſome of his acquaintances, who tried it with the ſame ſucceſs; and if the nuts be freſh, ſcarce any of them miſcarry. This led him to try, if the ſame method would ſucceed as well with other

other hard ſhell'd exotic ſeeds, which he could not, by any method he had before tried, get to grow, as the *bondus* or *nickar* tree; the *abrus*, or wild liquorice; the *phaſeolus braſilianus fruteſcens lobis villoſis pungentibus maximus Hermanni*, or horſe-eye bean, with ſeveral others; and he found it both a ſure and expeditious way to raiſe any ſort of hard ſhell'd fruits, or ſeeds: For, the heat and moiſture (which are abſolutely neceſſary to promote vegetation) they here enjoy in an equal and regular manner; the tanners bark, if rightly managed, keeping to near an equality of heat for ſix months; and the water, which deſcends from the pots, when watered, is detain'd by the bark from being too ſoon diſſipated; which cannot be obtain'd in a common hot-bed, the earth in ſuch being work'd away by the water, and thereby leaving the ſeeds often deſtitute of moiſture: Some of theſe ſeeds he has had ſhoot in a fortnight's time; which he is informed, would not have done ſo in a month in their native ſoil and climate. He alſo found this to be an excellent method to reſtore orange, or any other exotic, trees, which have ſuffered by a tedious paſſage, in being too long out of the ground. Inſomuch that he recover'd two orange-trees, which had been 10 months without either earth, or water.

The ſeveral Strata *of* Earths, *and* Foſſils, *found in digging the* Mineral-wells *at* Holt; *by Mr*, Lewis. Phil. Tranſ. N° 403, p. 489.

AFTER they had paſs'd the upper turf, they came to a blue clay, which held about three foot; then they met with a yellow, brittle clay, very much reſembling okre, us'd by painters, about two foot in thickneſs; and next this they met with a loam of a looſer texture, which ſparkled with a kind of talc, naturaliſts call *Selenites*, and intermix'd with yellow ocre. Theſe *Selenites*, which were found ſhot in the clay in conſiderable quantities, were cryſtals conſiſting of tranſparent, ſhining, brittle flakes, ſome of a rhomboidal, others of a conical figure, but all *hexaedra*, or columns of ſix ſides. They had no ſenſible taſte of ſalt, and the clay, in which they were found, was interſperſed with veins of colour'd earth, of the colour of ſulphur, and iron ruſt.

Below this, at about 10 foot deep, they came to a bed of ſtones, of a large ſize, and very hard texture, coated with flakes of *gypſum* of a white and yellowiſh colour, which run thro' and divide them by various membranes, as it were, into

into different cells, all fill'd with indurated loam of a grey colour. These stones, which were all of an oval figure, in shape resembling pebbles, weigh'd from 10 to 60 pounds, and lay all on a level one by another in the bed of clay. Here the springs come in, and below this the clay was darker colour'd, and interlaid with small shells of the oyster, escallop and muscle kind, and with a few belemnites, curiously shaped: Here they met with stones of a very close texture, which, when wash'd, seem'd to be nothing but a mass of shells jumbled and incorporated together: And a little lower the clay produced some lumps of a black, bituminous sulphur, interlaid with some small thin *laminæ*, seeming to be metalline, and bright like the purest silver: Upon firing this sulphureous bitumen on a red hot iron, it emitted a blue flame and strong smell like brimstone, but the metal was lost.

From this account of the different strata found in digging these wells, their impregnation seems to be from alum, vitriol of steel, ocre and sulphur: and from an accurate mixture of all these together, which no art can imitate, it seems to derive those admirable qualities with which it is endued.

Some conjectures may be formed of its nature and qualities from the tinctures it communicates upon chemical experiments: With astringent drugs, as galls, oak-leaves and balaustians, it sometimes tinges red, inclining to purple, and sometimes it will not tinge at all: With volatile alkali's, as spirit of urine and sal-armoniac it turns milky: With lixiviate salts, as oil of tartar and *deliq.* &c. it rises in a white curdle: But acid saline liquors, as spirit of salt, nitre, &c. cause no alteration.

A gallon and a half of this water being evaporated to dryness, what remained weigh'd 3 drachms 1 scruple and 19 grains; some parts of which were white, and shot into *striæ* like needles, and others into prisms.

The neighbouring country is chiefly a strong clay; the quarries produce a very hard stone, which seems to be a composition of shells, closely cemented, and incorporated together, and some marchasites which abound with sulphur: In sinking deep pits they throw up stones, resembling iron-ore, and cover'd with a shining metallic substance, and serpentine stones, &c. and the plow'd fields abound with stones, resembling shells of the escallop and cockle kind, striated with some astroites, which are all strong alkali's; and with *aqua fortis* or spirit of nitre raise a violent ebullition.

Concerning the Causes of the Gout; *by S.* Michele Pinelli. Phil. Transf. N° 403. p. 491.

IN order to acquire a competent knowledge of the various distempers, incident to the human body; particularly such whose causes not being very well known, their cures have also hitherto been found very difficult, it is absolutely necessary, that by the concurrent assistance of reason, and experience, we should make an accurate enquiry, into the principles of which it is compos'd.

With this general view, but more particularly, in order to discover the nature and cause of the gout, S. *Pinelli* had for some time applied himself to examine the several parts of the human body, whether solid or fluid, and by chemical processes to dissolve them into their primitive or constituent parts.

Thus, for instance, he found, that the solid substance of the bones and the circumambient parts, is compos'd of phlegm, oil, an alcaline volatile salt, and a little earth. He also constantly discover'd the same principles in the blood, and all the other fluids, which are separated from it; as well as the other solid parts, with this difference only, that there is more phlegm, less oil and volatile salt in the fluids, than may be extracted from the solids.

Carrying these experiments still farther, and endeavouring to extract *viâ humidâ* (as chemists call it) the salt from the excrements in the stomach and intestines, he constantly found a salt not unlike sea-salt, and upon examination appearing to be a perfect acid.

And yet, with all the care and accuracy possible, nothing of this acid salt could ever be obtain'd from all the other solid, or fluid parts of the body.

Hence it appears how much those must be mistaken, who have hitherto asserted, that the cause of the gout is a coagulating acid existing in the blood; since nothing of an acid is found in any part of the body, but the excrements. This put S. *Pinelli* upon thinking, that the cause of the gout might be owing, perhaps, to that other saline principle, which chemical trials shew us to exist both in the solid, and fluid parts; and he was confirmed herein by the following experiments.

A very gouty person having died at *Rome*, S. *Pinelli* procur'd three ounces and a half of that tophaceous gouty substance

ſubſtance, commonly found about the joints of perſons, afflicted with this diſtemper; and taking ſix glaſs bottles, he put ten grains of the ſaid ſubſtance into each: The firſt of theſe bottles he fill'd with diſtill'd vinegar, the ſecond with ſpirit of vitriol, the third with ſpirit of ſalt, the fourth with ſpirit of ſal-armoniac, the fifth with ſpirit of hartſhorn, and the ſixth with ſpirit of urine: After 24 hours he found the aforeſaid tophaceous matter entirely diſſolv'd in the three firſt bottles, which contain'd the acid ſpirits; but in the others, which he fill'd with alcaline ſpirits, it remain'd entire and untouch'd, even for ſome time after. Hence he concluded this tophaceous matter to be of an alcaline nature, in regard that it is the nature of acid ſpirits to diſſolve ſuch ſubſtances, as are either altogether alcaline, or compos'd in part of an alkali: And this in like manner he conceiv'd to be the reaſon why the aforeſaid tophaceous ſubſtance remain'd entire in the bottles fill'd with alcaline ſpirits, as being both of the ſame nature; and conſequently, not to be diſſolv'd by each other.

But for farther ſatisfaction he took the remaining part of this tophaceous matter, being about three ounces, and put it into a ſmall retort: Then having fix'd a recipient to it, he diſtill'd it S. A. by a gradual fire, and obtain'd a ſpirit, with ſome few drops of oil, about two drachms of a *caput mortuum* remaining in the retort. This ſpirit he found to be a perfect volatile alcali, altogether of the ſame nature with that extracted from the blood, from urine and from bones. Hence again it is evident, that this tophaceous gouty ſubſtance is compos'd of the ſame principles, with the other fluid, and ſolid parts of the human body; or that the cauſe of the gout is nothing elſe but a volatile, alcaline, corroſive ſalt, which by corroding the ſenſible membranes about the joints, occaſions thoſe acute pains we call the gout.

Of foſſile Teeth *and* Bones *of* Elephants; *by Sir* Hans Sloane. Phil. Tranſ. N° 404. p. 497.

HERE Sir *Hans Sloane* offers ſome remarks on divers accounts of bones, and teeth found under ground, which he met with in ſeveral ancient and modern authors, and which give him an opportunity of examining into the skeletons, and parts of skeletons, which are ſhewn up and down, as undeniable monuments of the exiſtence of giants.

And firſt, as ſeveral of thoſe bones and teeth, which are kept and ſhewn about for bones and teeth of giants, have been found, upon a more accurate inſpection, to be only the bones and teeth of elephants, or whales, it may from thence very probably be inferr'd, that others alſo, which for want of a ſufficient deſcription cannot be accurately enough accounted for, muſt have belonged either to thoſe or ſome other large animal. Thus the fore-fin of a whale, ſtripp'd of its web and skin, was not long ago publickly ſhewn for the bones of a giant's hand; and Sir *Hans* has in his own poſſeſſion (N° 1027) the *vertebra* of the loin of a large whale, which was brought him from *Oxfordſhire*, where he was aſſur'd it was found under ground, and afterwards made uſe of for a ſtool to ſit on. Now if a computation had been made from the proportion of this *vertebra* to that of the other parts of the skeleton, and all had been ſuppos'd to have belonged to a man; ſuch a skeleton would have exceeded in meaſure all thoſe fabulous skeletons of giants, mentioned by authors.

Sir *Hans Sloane* cannot on this occaſion forbear to obſerve, that it would be an object well worthy the enquiries of ingenious anatomiſts to examine, with more accuracy than has hitherto been done, what proportions the skeletons and the parts of skeletons of men and other animals bear to each other, with regard either to the ſize, figure, ſtructure, or any other quality. This would, undoubtedly, lead us into many diſcoveries, and beſides, is one of thoſe things, which ſeem to be wanting to make anatomy a ſcience ſtill more perfect, and compleat. The very *vertebra* Sir *Hans* ſpeaks of may ſerve to ſhew the uſefulneſs of ſuch obſervations: It differs in ſeveral particulars from the *vertebræ* of men, and land animals; as do the *vertebra* of whales and the fiſhes of the cetaceous kind in general, and it is a very eaſy matter to diſtinguiſh them from each other. The body of the *vertebra* is conſiderably larger in proportion, and alſo lighter and more porous. The tranſverſe proceſſes ariſe from the middle of it on each ſide. The oblique deſcending proceſſes are altogether wanting; and the arch, or *foramen*, which the ſpinal marrow paſſes thro', is made up by the ſpinal proceſs, and the oblique aſcending ones only: The body of the *vertebra* is very rough and uneven on each end; full of ſmall holes and eminencies, which receive the holes and eminencies of a round bone, or plate; which anſwers to the epiphyſis in a human *vertebra*, whereof there are two between each *vertebra*,

joined

joined together by an intermediate ſtrong, and pretty thick cartilage, probably, to facilitate the motion, and particularly, the flexion of theſe animals in the ſea. But to return.

There are ſeveral ſkeletons, which from time to time were found under ground, and are mentioned by the authors, who ſpeak of them, as ſkeletons of giants, and undeniable monuments of their exiſtence; which, as Sir *Hans* has already obſerved, he would rather take to be the ſkeletons of elephants, whales, or ſome other huge land, or ſea animal. Of this kind ſeem to be the pretended ſkeletons of giants of 12, 20, and 30 cubits high, mentioned by *Philoſtratus* in *ſuis Heroicis*. The ſkeleton of 46 cubits in height, which according to *Pliny Hiſt. Nat. lib.* 7. *cap.* 16. was found in the cavity of a mountain in *Crete*, upon the overthrowing of that mountain by an earthquake: The ſkeleton 60 cubits high, which *Strabo lib.* 17. ſays, was found near *Tingis* (now *Tangier*) in *Mauritania*, and ſuppoſed to have been the ſkeleton of *Anteus*: The ſkeleton of *Pallas*, as is pretended, found at *Rome Anno* 1500, that was higher than the walls of that city; and likewiſe that, which *Simon Magolus Dierum canicularium Colloq.* 11. p. 36. ſays, was found in *England Anno* 1171, his words are to the following purpoſe; 'long before *Fulgoſus*'s time, upwards of '300 years, namely *Anno* 1171, by the overflowing of a river, 'there was diſcovered in *England* a human ſkeleton, where 'the bones were ſtill in their proper order: The length of the 'whole body was found to be 50 feet.'

There are others, the deſcription of which concludes more clearly for their having once belonged to elephants, tho' it could not be poſitively aſſerted, that they really did. St. *Auſtin* (*de civit. Dei lib.* 15. *cap.* 9. as cited by *Caſſanio* and *Lambecius*) ſpeaking of the exiſtence, and great feats of the giants before the deluge, mentions in proof of what he advances, that he himſelf, with ſeveral others ſaw at *Utica*, upon the ſea ſhore the grinder of a man ſo large, that if it had been cut into teeth of an ordinary ſize, at leaſt an hundred might have been made out of it. *Hieronymus Magius, Miſcellan. lib.* 1. *cap.* 2. *p.* 17. tho' himſelf very much prejudic'd in favour of the exiſtence of giants, yet ſuſpects this tooth, mentioned by St. *Auſtin*, to have been rather the tooth of an elephant, or ſome huge ſea animal, than that of a man. But *Ludovicus Vives* in his *commentaries* on that paſſage of St. *Auſtin*, takes notice, that in the church of St. *Chriſtopher* at *Hiſpella*, he was ſhewn a tooth bigger than his fiſt, which they pretended was one

one of the teeth of that huge ſaint; and no doubt, upon as good grounds, as that huge ſhoulder-bone, which *Hieronymus Magius* ſays in *loc. cit.* p. 20. 6. was ſhewn in a church at *Venice* as the ſhoulder-bone of St. *Chriſtopher*.

The pretended ſkeleton of a giant, which was found near *Drapani*, a caſtle in *Sicily*, upon digging the foundations of a houſe, and deſcribed by *Joh. Boccatius Genealogia degli Dei lib.* 4. *ad. fin.* is again not unlikely to have been the ſkeleton of a large elephant. For, tho' the greateſt part of the bones, thro' length of time, and the force of ſubterraneous ſteams, were ſo rotten, that after their being expoſed to the air, they fell to pieces almoſt upon touching; yet three of the teeth were found entire, which weighed an hundred ounces, and were by the inhabitants of *Drapani* hung up in one of their churches, to perpetuate the memory of this fact. They likewiſe found part of the ſcull, that could contain ſome buſhels of corn, and one of the ſhank-bones, which was ſo large, that upon comparing it with the ſhank-bone of an ordinary man, it was judged, that this giant whom ſome took to be *Eryx*, others *Enceladus*, others one of the *Cyclopes*, and others again the renowned *Polyphemus* himſelf, muſt have been 200 cubits high; according to which calculation, he is delineated and repreſented by F. *Kircher Mund. ſubterran. lib.* 8. *ſect.* 2. as by far the largeſt of a whole gradation of giants, whom, after this, he places in the following order.

	Cubits
The giant of *Strabo*, whoſe ſkeleton was dug up near *Tingis* in *Mauritania*, and was found to be	60 high
Pliny's giant found in a mountain in *Crete*	46
The ſkeleton of *Aſterius*, ſon of *Anactes*	10
The skeleton of *Oreſtes*, dug up by ſpecial command of the oracle	7
The giant, whoſe bones were found under a large oak, not far from the convent of *Reyden*, in the canton of *Lucern* in *Switzerland*	9
Goliah, as deſcribed in *Sacred Writ*.	6 ½

The caſe is ſtill leſs dubious with regard to thoſe bones, which were found in *France Anno* 1456, in the reign of *Charles* VII. by the ſide of a river in the barony of *Cruſſole* (afterwards erected into a county) not far from *Valence*. *Johannes Marius in libris de Galliarum illuſtrationibus*, *Calamæus in ſuis de Biturigibus commentariis*, *Fulgoſus* in his *Annals*, and

and *John Cassanio* of *Monstroeuil* in his *treatise of giants* p. 57. and *seq.* severally take notice of these bones, which were so large, that the whole height of the giant, to whom it was thought they belonged, and who was supposed to have been the giant *Briatus*, was conjectured to have been 15 cubits high: The scull alone was two cubits thick, and the shoulder bone six cubits broad. Some time after, other bones of this kind were found in the same barony near the same place, part of which *Cassanio* himself saw, and gives such a particular description of one of the teeth, as leaves little room to doubt, but that it was the grinder; and consequently, the other bones, those of an elephant: His words are in p. 62. to the following purpose; 'I saw there several bones, among which was a tooth 'of a surprising bigness, one foot long, and weighing eight 'pounds; it seemed to be much longer than it was thick, and 'to have some roots by which it was fastened in the jaw: Besides, that part by which the food is ground, appeared somewhat concave and four inches broad.' He adds farther, that such another tooth was kept at *Charmes*, a neighbouring castle; that he measured the length of the place, whence these bones were dug, and found it to be nine paces; that some time after, more bones were discovered at the same place; and that the country all round was very mountainous, and such, as the giants, in all probability, delighted to dwell and command in. Sir Hans Sloane had seen some of these bones, brought by a *French* merchant from this last mentioned place, which the former took to have belonged to an elephant, by some large cells between the tables of the scull, which are in the scull of that animal.

Hieronymus Magius Miscellan. lib. 1. *cap.* 2. p. 19. 6. gives an account of a very large scull, 11 spans in circumference, and some other bones, probably, belonging to that scull, that were dug up near *Tunis* in *Africa* by two *Spanish* slaves, as they were plowing in a field: He was informed of this matter by *Melchior Guilandinus*, who saw the scull himself, when taken by the rovers, and carried into slavery to that place *Anno* 1559. Sir *Hans Sloane* is the more apt to believe, that this scull and bones were part of the skeleton of an elephant; because, as shall be shewn hereafter, a like large skeleton was dug up near the same place some time after, which by one of the teeth sent to *Peiresk* was made out to have been the skeleton of an elephant.

Sir *Hans* comes now to those bones, teeth, and tusks (or horns, as some call them) which are mentioned by authors to have

have been dug up in divers parts of the world; and have been made out by them, or do otherwiſe appear by their deſcription and figures, indiſputably to belong to the elephant.

Johannes Goropius Becanus Originum Antwerpianarum lib. entitul'd *Gigantomachia p.* 178. notwithſtanding he lived in an age, when the ſtories of giants were very much credited, and had found their advocates, even among perſons eminent for their learning and judgment, yet ventured to aſſert, that the tooth which was kept and ſhewn at *Antwerp*, as the tooth of that unmerciful giant, whoſe defeat, brought about as they pretended, by *Brabo* a ſon of *Julius Cæſar*, and king of the *Arcadians*, was fabulouſly reputed to have given occaſion to the building of that caſtle and city, was nothing but the grinder of an elephant. However diſpleaſing this aſſertion might be, as *Goropius* farther adds, to thoſe who are delighted with ſuch idle and ridiculous ſtories; yet to the judicious it will appear the leſs ſurpriſing, on account of what paſt not long before he wrote this book, when the almoſt entire ſkeletons of two elephants with the grinders, as alſo the *dentes exerti* or tusks, were found near *Wielworda*, *Vilvorden*, as they were digging a canal from *Bruſſels* to the river *Rupel* to defend that town and country from the incurſions of thoſe of *Mechlen*. *Goropius* conjectures, that theſe elephants had been brought thither by the *Romans*, at the time either of the emperor *Galien*, or *Poſthumus*.

A very large ſkeleton of a giant, as pretended, was alſo dug up near *Tunis* in *Africa* about the year 1630, whereof one *Thomas d' Arcos*, who was then at that place, ſent an account, together with one of the teeth, to the learned *Peiresk*: The ſcull was ſo large, that it contained 8 *meilleroles* (à meaſure of wine in *Provence*) or 1 *modius*, as *Gaſſendus* calls it *in vitâ Peiresk lib.* 4. *Anno* 1632, or a pint and a half *Paris* meaſure. Some time after a live elephant having been ſhewn at *Toulon*, *Peiresk* ordered he ſhould be brought to his country ſeat, on purpoſe to take that opportunity to examine the teeth of the creature, the impreſſions whereof he cauſed to be taken in wax; and thereby he found, that the pretended giant's tooth ſent him from *Tunis*, was only the grinder of an elephant. This is the ſecond large ſkeleton dug up near *Tunis* in *Africa*, and it appearing plainly by the tooth ſent to *Peiresk*, that it was the ſkeleton of an elephant, it may from thence very probably be conjectured, ſome other circumſtances concurring, that the other

also, which *Guilandinus* saw there, must rather have been that of an elephant, than of a giant.

Thomas Bartholin Act. Medic. & Philosoph. Hafn. Tom. I. Obs. 46. *p.* 83. mentions the grinder of an elephant, which was dug up in *Iceland*, and sent to him by *Petrus Resenius*: It was turned to a perfect stony substance, like flint, as was also the tusk of a *rosmarus* dug up in *Iceland*.

A large tooth, which by its shape appears plainly to be the grinder of an elephant, is described, and figured by *Lambecius, Biblioth. Cæsar. Vindob. lib.* 6. *p.* 311. who had it out of the emperor's library, tho' he could not be informed where it was found, or how it got thither: It weighed 28 ounces, and was commonly taken to be the tooth of a giant. *Antoninus de Pozzis*, chief physician to the emperor, in a letter to *Lambecius, ibid. lib.* 6. *p.* 315. affirms it is an elephant's tooth, and conjectures, that it was dug up at *Baden*, about four miles from *Vienna*, where, but a few years before he wrote this letter, they had also found the *os tibiæ* and *os femoris* of an elephant.

Another tooth, and probably, too of an elephant, is described and figured by *Lambecius, ibid. lib.* 6. *p.* 313. who had it out of the emperor's library: It weighed 23 ounces, and was found in the year 1644 at *Krembs* in the lower *Austria*, as they were enlarging the fortifications of that place.

The year following, when the *Swedes* came to besiege the town of *Krembs*, an entire skeleton of a giant, as was pretended, was found at the top of a neighbouring mountain, near an old tower. The besiegers in their intrenchments there, being very much incommoded by the water that came down from the mountains, dug a ditch three or four fathoms deep, to convey it another way. It was in digging this ditch they found the aforesaid skeleton, which was very much admir'd for its unusual size: Several of the bones, chiefly those of the head, fell to pieces upon being exposed to the air, and others were broken by the carelessness of the workmen: Some escaped entire, and were sent to learned men in *Poland* and *Sweden*. Among these was a shoulder-bone, with an *acetabulum* therein, large enough to hold a cannon ball. The head, with regard to its bulk, was compared to a round table; and the bones of the arms, or fore-legs, as thick as a man of an ordinary size. One of the grinders, weighing five pounds, was given the *Jesuits* at *Krembs*; another is figured by *Happelius* in his *Relationes curios. Tom.* 4. *p.* 47, 48. from whom Sir *Hans Sloane* has this account, and it plainly appears by the figure of it, that it is an

elephant's tooth. It weighed four pounds, three ounces *Nu-remburg* weight.

Again, in *Lambecius's Biblioth. Cæſar. Vindobonenſis lib.* 8 *p.* 652. are two figures, and the deſcription of a very large ele-phant's tooth, which weighed four pounds and $\frac{3}{4}$. It was ſe[nt] from *Conſtantinople* to *Vienna* in 1678, and offered to be ſol[d] to the Emperor for 2000 rix dollars, having been before, for i[ts] unuſual ſize, and pretended great antiquity, valued at 10,00[0] rix dollars. They pretended that it was found near *Jeruſalem* in a ſpacious ſubterraneous cavern, in the grave of a giant, whic[h] had the following inſcription upon it in the *Chaldaic* languag[e] and characters; *Here lies the Giant* OG: Whence it was con-jectured to have been the tooth of *Og*, king of *Baſhan*, wh[o] was defeated by *Moſes*, and *who only remained of the remnan[t] of giants; whoſe bed-ſtead was of iron,* 9 *cubits was th[e] length thereof, and* 4 *cubits the breadth of it, after the cub[it] of a man*, Deut. ch. 3. v. 2. As the whole ſtory looked ver[y] like an impoſition, the emperor ordered, that the tooth ſhoul[d] be ſent back again to *Conſtantinople*.

Hieronymus Ambroſius Langenmantel, a member of the *Im-perial Academy of Sciences*, inſerted into the *Ephemerides* [of] that academy an abſtract of a letter to himſelf *Decur.* [2.] *Ann.* 7. 8. 1688. *Obſ.* 234. *p.* 446. from *Johannes Ciampina* [at] *Rome*, concerning ſome very large bones, to wit, the ſhank bone, ſhoulder-bone and five *vertebræ* (one of which was a *ver-tebra* of the neck) which were dug up near *Vitorchiani* in th[e] biſhopric of *Viterbo* in the year 1687. They all togethe[r] weighed upwards of 180 *Roman* pounds; and compared wi[th] other the like bones in ſeveral collections at *Rome*, particularl[y] the *Chiſian* one, they appeared to be by far the largeſt. Mo[st] people took them to be the bones of a giant; but *Ciampin[i]* and ſome others, taking them, with more probability, for th[e] bones of an elephant, or ſome other large animal, and knowin[g] that there was in the medicean collection at *Florence* a comple[at] ſkeleton of an elephant, they procured a draught of it, an[d] found upon compariſon the above-mentioned bones ſo exactly [to] correſpond with it, as to leave no room to doubt, but that the[y] had been part of an elephant's ſkeleton.

The ſkeleton of an elephant, dug up in a ſand-pit near *Tonn[a]* in *Thurengen* in 1695, is one of the moſt curious, and likewi[ſe] the moſt compleat in its kind; foraſmuch as they found th[e] whole head, with four grinders, and the two *dentes exerti*, [or] tuſks, the bones of the fore and hind-legs, one of the ſhoulde[r]

bone

bones, the back-bones, with the ribs, and ſeveral of the *vertebræ* of the neck. But the whole hath been ſo accurately deſcribed by *Wilhelmus Erneſtus Tentzelius*, Hiſtoriographer to the Dukes of *Saxony*, in a letter to the learned *Magliabechi*, publiſhed in *Phil. Tranſ.* N° 234. that it is needleſs to add any thing, the rather as that Gentleman preſented the *Royal Society* with ſome pieces of the bones of this elephant, with part of the ſcull, wherein appeared its cells, ſome of the grinders, and part of the *dentes exerti*, or tuſks; all which were found exactly agreeable to his deſcription, and ordered to be carefully preſerved in the *Royal Society*'s repoſitory. From the ſurface of the ground down to the place, where theſe bones were found, the diſpoſition of the *ſtrata*, or layers, was as follows; a black ſoil four foot deep; gravel two foot and a half, the middle of which was made up of *oſteocolla* and ſtones to the depth of two foot; *oſteocolla* and ſtones half a foot; and ſandy clay ſix foot, with about two inches of *oſteocolla* in the middle; *oſteocolla* and pebbles one foot; gravel ſix foot; a white and fine ſand, whoſe depth was unknown, and in this the bones were found.

In Vol. 2. of Count *Marſili's Danubius*, where he treats of the antiquities he obſerved along this river, there is mention made of ſeveral bones and teeth of elephants, which that inquiſitive nobleman met with in *Hungary* and *Tranſilvania*, and which are now in his valuable collection of natural and artificial curioſities at *Bologna*. According to the beſt information the people, of whom he had them, could give him, they were found in rivers, lakes and pools. One of the *vertebræ*, a grinder, and a conſiderable part of the *dens exertus*, or tuſk, were found in the lake, or pool of *Hiulca*. Two fragments of the *os tibiæ*, a little corroded on the inſide, were taken out of a pool near *Togheras* in *Tranſylvania*, once the ſeat of the princes of that country; and the whole lower jaw, with two grinders ſtill ſticking in it, he had from ſome fiſhermen, who found it in the ſtanding waters by the river *Tibiſcus*, a little above *Die Römer ſchantz*, or the *Roman* fort. All theſe the author caus'd to be delineated as big as the life.

The opinion of *Goropius* about the antiquity of thoſe two elephants, the skeletons of which were found near *Vilvorden*, which he traces no higher than the time of the *Romans*, and their expeditions into thoſe countries, particularly under *Galien* and *Poſthumus* was related above. Count *Marſili* is of the ſame opinion with regard to thoſe bones, and teeth, found by

him in *Transylvania*: He takes notice, that whoever is acquainted with the vast use the *Romans* made of elephants in their military expeditions, ought not to be surprised, that there are bones and teeth of them found in those northern countries, where otherwise there could not have been any; and he urges as a farther proof of this assertion, that they are found in pools and lakes, it having been the custom of the *Romans*, to throw the carcasses of dead elephants into the water (as it is still practised to this day) with the carcasses of horses and other beasts, to prevent the distempers and other inconveniencies, which their putrifaction might otherwise occasion.

On the other hand there are several arguments, taken from the largeness of the beasts, whose skeletons are thus found under ground, which sometimes far exceeds any that was, or could have been brought alive into *Europe*, from the condition they are found in, and from the particular disposition of the *strata* above the places where they are found; whereby it appears, almost to a demonstration, that they must be of much greater antiquity, and that they could not have been buried at the places where they are found, or brought thither any otherwise than by the force of the waters of an universal deluge.

To insist only upon one of these arguments: If the skeletons of elephants, which are thus found under ground, and at considerable depths too, had been buried there either by the *Romans*, or any other nation, the *strata* above them must necessarily have been broken thro' and altered; whereas on the contrary, several observations inform us, that they were found entire: Whence it evidently appears, that what is found underneath, must have been lodged there, if not before, at least at the very time when these *strata* were formed; consequently, long before the *Romans*.

But there is another argument, which appears to Sir *Hans Sloane* to bear very hard against the conjectures of *Goropius* and Count *Marsili*: *Tentzelius* has already mentioned it, and it is urged from the great value of ivory at all times, and particularly among the *Romans*, which appears by several passages in ancient authors; as for instance, by a very remarkable one in *Pliny lib.* 12. *cap.* 4. who takes notice, that among the valuable presents, which the *Ethiopians* were obliged to make to the kings of *Persia*, by way of tribute, there were 20 large teeth (unquestionably the *dentes exerti*) of elephants; and then adds, *tanta ebori authoritas erat.* Now it is to be presumed, that the *Romans* would not have neglected to take away the teeth,

teeth, and particularly the *dentes exerti* of dead elephants, before they flung their carcases into the water; whereas there hath scarce been any skeleton, or any part of the skeleton of an elephant, dug up any where, but the teeth were found along with them, and even among those figur'd by Count *Marsili* there are three grinders, and a considerable part of one of the *dentes exerti*.

Dr. *Plott* in his *Natural History of Staffordshire* ch. 7. §. 78. p. 78. says, that *William Leveson Gower* of *Trentham* Esq; made him a present of the lower jaw of some animal, with large teeth sticking in it, dug up in a marle-pit in his ground, and which upon comparison he found exactly agreeable to the lower jaw of the elephant's scull in the *Asmolean Musæum* at *Oxford*.

In the *Musæum* of the *Royal Society* there are two fossile bones of elephants; one was given by Sir *Thomas Brown* of *Norwich*; the other was brought from *Syria* for the *os tibiæ* of a giant; but Dr. *Grew*, *Musæum Reg. Soc. p.* 32. proves by an exact computation, that it could never have been the *os tibiæ* of a human skeleton, by being full 20 times as thick, and but three times as long: It is an *English* yard and half a foot in length, and a foot in circumference, where thinnest. Dr. *Grew* observes, that by the figure it appears to have belonged to the leg, and not to the thigh; and he conjectures the whole elephant to have been about five yards high.

Before Sir *Hans Sloane* dismisses this subject, he mentions a few more: *Gesner de figuris lapidum* p. 157. takes notice, that a *Polish* nobleman presented him with a tooth four times as large as that, which he delineated under the title of *Hippopotamus* in his book *de aquatilibus*: It was found under ground, as they were digging for the foundation of a house, together with a very large horn, as they call'd it, which several took to be a unicorn's horn; but erroneously as *Gesner* thought, because of its being too thick, and too crooked: It is very probable that this pretended horn was the *dens exertus* of an elephant. The same author mentions a subterraneous cavern near *Elbingeroda*, wherein were found the bones and teeth of men and other animals, so large, that it was scarce credible, that any of that bulky size should ever have existed.

The petrified grinder of an elephant is kept in the King of *Denmark*'s cabinet at *Copenhagen*, as appears by the catalogue *Mus. Regium* part. 1. §. 7. N° 109. but there is no mention made how it came thither, or where it was found.

They

They ſhew in the ſame collection a large thigh bone, which weighs about 20 *Daniſh* pounds, and is upwards of three foot in length, *ibid.* part. 1. §. 1. N° 73. It is ſo old, according to the author of the catalogue, that it is almoſt become ſtony. The ſame author takes notice of another large bone, in the collection of *Otho Sperling*, that weigh'd 25 pounds, and was four foot long. It was, as *Sperling* told him, found *Anno 1643* at *Bruges* in *Flanders*, near the public priſon, in preſence of *Bernard de Arauda*, and *Sperling*'s father, who ſaw the whole skeleton there, which was 20 *Brabant* yards in length.

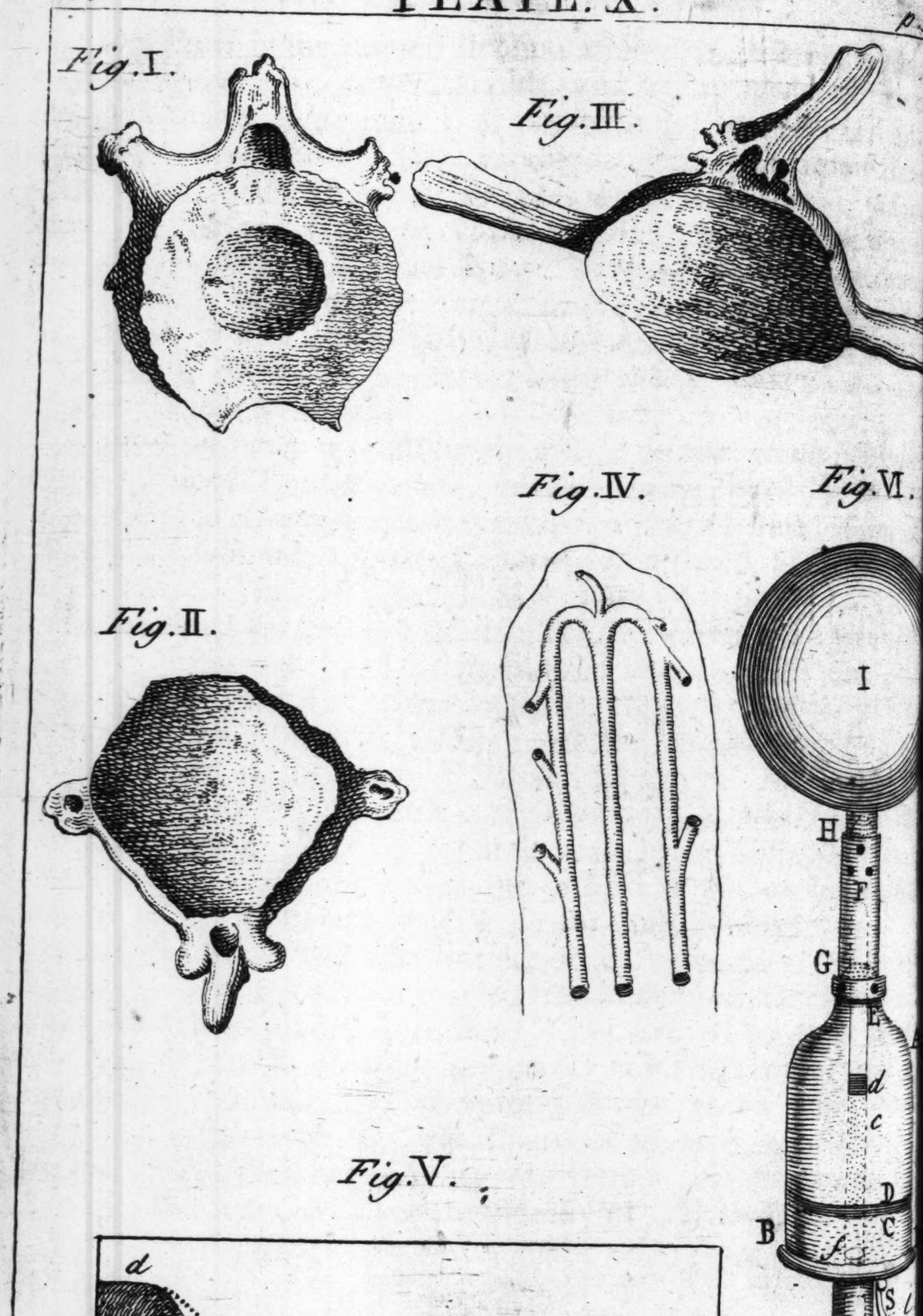
A piece of ivory was dug up in a field on the river *Viſtula* about ſix miles from *Warſaw*, which having been ſhewn at *Dantzick* to *Gabriel Rzaczynski*, author of the *Natural Hiſtory of Poland*, appeared to him to be the *dens exertus* of an elephant. Vide *Rzaczynſ. Hiſt. Nat. Reg. Polon. p.* 2.

In the notes on the laſt edition of Dr. *Herman*'s *cynoſura Medica*, publiſh'd by Dr. *Boecler* of *Strasburg*, 1726. 4to. *P.* 3. *p.* 133. entituled *Unicornu foſſile*, there is mention made of a remarkable piece of foſſile ivory, or rather of an elephant's tooth, in the hands of *Jaques Sanſon de Rathambauſen de Ehenweyer*, an *Alſatian* nobleman. It was found in the *Rhine* upon one of his eſtates near *Nonneville*, and was three *Paris* feet, three inches and a half long; at the baſis where thickeſt, it was near a foot in circumference; and about eight inches and a half at the other extremity. It was fill'd within with a ſort of marle, but the outer ſurface was ſtony in ſome places, and bony in others: The bony part ſcraped, or burnt, ſmell'd like ivory: The ſcrapings boil'd made a kind of jelly. The author of the notes adds, that they find foſſile ivory in ſeveral parts of *Europe*, particularly in the *Schwartzwald (Sylva Hercynia)* in *Moravia*, in *Saxony*, and near *Canſtad* in the dutchy of *Wirtemberg*.

Obſervations on the Stomachs of Oxen; *by Mr.* Charles Price. Phil. Tranſ. N° 404. p. 532.

IN the ſtomach of a cow Mr. *Price* finds two things well worth obſerving. 1. That the *villi* compoſing the villous coat (which in men are ſo very ſmall, as to be ſcarce viſible when examined ſeparately) are in this animal ſo very large, as to admit of an exact ſcrutiny into their ſtructure: Each *villus* is formed by a duplicature of the internal *lamina* of the vaſcular coat, from which it receives three blood-veſſels, as repre-

Vol. VIII. PLATE. X.



G. Smith del. Hulett sculp.

repreſented Fig. 4. Plate X. which ſhews one of the *villi* of an ox's ſtomach, magnified: Whether the two ſide-veſſels are arteries, and the middle veſſel a vein; and whether thoſe ſmall branches, ariſing from the ſide veſſels, are ſecretory ducts, conveying a fluid from theſe arteries into the cavity of the ſtomach, and forming a kind of rivulet, as it were, perpetually running thro' the *ductus alimentalis*, he leaves others to determine.

The other thing remarkable in the ſtomachs of theſe large animals is, that their internal ſurface is cover'd by a production of the cuticle, which deſcends from the lips quite thro' the alimentary paſſage. Mr. *Price* is apt to think, that the cuticle is continued thro' the inteſtines, as well in men, as in large animals; tho' its exceeding fineneſs may make it leſs obſervable.

Eclipſes *of* Jupiter's Satellites, *obſerv'd at* Bologna *in* 1727; *by* S. Manfredi; *as alſo* Eclipſes *of* Jupiter's *firſt* Satellite *obſerv'd at* Liſbon *the ſame Year*; *by* F. Carbone. Phil. Tranſ. N° 404. p. 534. *Tranſlated from the* Latin.

Jan. 2 1727 N. S.	9^h	45′	47″	The immerſion of *Jup.* III. ſatellite, obſerv'd with a 14 foot teleſcope.
	11	53	38	Its emerſion: Doubtful.
5	6	51	54	Emerſion of his I ſatellite with a very clear teleſcope of 11 *Bononian* feet
7	8	54	12	Emerſ. of the I ſat. with the ſame tel.
Feb. 7	5	50	5	Immerſion of the III ſatellite with the ſame teleſcope.
	7	52	54	Emerſ. of the III with a 14 foot teleſ.
8	8	37	59	Emerſion of the II with 11 foot teleſ. The air ſomewhat foggy.
Aug. 21	13	34	39	Immerſ. of the I with the ſame teleſ.
Sep. 6	11	55	15	Immerſ. of the I with the ſame teleſ.
17	10	48	59	Immerſion of the I with the ſame teleſcope: Doubtful.
	12	40	50	Emerſion of the I with the ſame teleſcope: Somewhat doubtful.
Oct. 15	9	10	54	F. *Carbone* with a 22 foot teleſcope obſerv'd an immerſion of *Jupiter's* innermoſt ſatellite.
Nov. 7	9	25	47	An immerſion of the innermoſt ſatellite with the ſame teleſcope.

An

An Occultation *of* Venus *by the* Moon, *at* Bononia, Sep. 18 1727, *N. S. by* S. Manfredi. Phil. Transf. N° 404. p. 535. *Translated from the* Latin.

	h.	′	″	
Sep. 18 1727	0	27	21	*Venus* begins to be hid behind the obscure limb of the moon.
	0	28	13	*Venus* entirely immerged.
	1	16	45	*Venus* begins to emerge out of the bright limb of the moon.
	1	17	50	Entirely emerged.
	1	46	24	Now the preceeding illuminated limb of the moon preceeds *Venus* in a horary circle 21″ of time; and the northern limb of the moon, likewise illuminated, is more southerly than *Venus* by 29″ of time.

The Barometrical Method of measuring the Height of Mountains, *together with two new Tables, shewing the Height of the* Atmosphere *at given Altitudes of the* Mercury; *by Dr.* J. G. Scheuchzer. Phil. Transf. N° 405. p. 537.

THE height of mountains, and their elevation above the level of the sea, have at all times been thought worthy the attention of inquisitive philosophers.

We find in *Pliny Nat. Hist. lib.* 11. *c.* 65. that *Dicæarchus* one of the old geographers, a disciple of *Aristotle*; and as *Pliny* himself calls him, a man of great learning, had by particular order of some princes measur'd the heights of several mountains, and that the highest of them, mount *Pelius* in *Thessaly* was found by his observations 1250 paces perpendicular height.

Cleomedes also, a *Grecian* astronomer and geographer, who liv'd some time before our Saviour's Nativity, asserts *Cyclica Theor. cap.* 10. that the highest mountain cannot exceed 15 *stadia*, or 9375 *Roman* feet.

But *Plutarch* in *Vitâ Æmilii* fixes the perpendicular height of the highest mountains; as also the greatest depth of the sea, only to 10 *stadia*, or 6250 *Roman* feet.

By the sequel of this transaction it will appear, that the height of mountains, as determined by these early writers, does not so very much deviate from truth, as one would be apt to suspect

ſuſpect from the infant ſtate of arts and ſciences in thoſe imes; particularly the 15 *ſtadia* of *Cleomedes*, which make out 9375 *Roman*, or 10316 *Paris* feet, will be found by the ollowing obſervations to come very near the height of the mountains of *Swiſſerland*; which, tho' the higheſt in *Europe* do not riſe above 10,000 *Paris* feet above the level of the ea; and it may ſeem ſurpriſing, that ſubſequent writers, even ſuch as were otherwiſe deeply ſkill'd in mathematical earning, ſhould run them up to an extravagant, and altogether unnatural height.

At firſt, it is not improbable, that they went only upon bare conjectures; but afterwards when geometry came to be more and more improv'd, quadrants, ſemi-circles, and other geometrical inſtruments were brought into uſe; by means of which, and by a trigonometrical calculation, the heights of places could be determined in a more ſatisfactory manner: And yet, however true the principles be, upon which this method is founded, however nice the inſtruments, and however curious the obſerver, the method itſelf muſt be owned, and hath been found by undoubted experiments, to fall much ſhort of that accuracy, which it ſeems to promiſe; and the more conſiderable the heights, the more uncertain it will prove.

For in the firſt place, as the ſtate of the air is very different in different ſeaſons and different weather, its refraction does alſo thereby become conſiderably alter'd, which occaſions the tops of mountains to appear higher at ſome times, than they do at others; and at all times higher than they really are. But beſides, there is another inconveniency, which, whoever is acquainted with the true ſtate of mountainous countries, muſt needs be ſenſible of, and that is the extreme difficulty of meeting at the bottom of high mountains with plains large enough for a proper horizontal ſtand, or baſis to ſuch a triangle, as an accurate and knowing obſerver would think ſatisfactory to determine a conſiderable height, making even proper allowances for the refraction of the air.

Among the many improvements in natural philoſophy, which are owing to the *Toricellian* tube, one of the moſt conſiderable inventions of the laſt century, it hath thereby been enriched with a new method of meaſuring the reſpective heights of places, and their elevation above the level of the ſea; a method, which, tho' it muſt be own'd, has not hitherto and perhaps, conſidering the inconſtancy of the air, hardly

ever will be brought to an absolute degree of certainty, is yet in many respects preferable to the trigonometrical one, as it has likewise been found by experience to come nearer the truth, and leads us, by a new and singular scale, from the horizon of the sea to the tops of the highest mountains, a distance far beyond the reach of geometrical instruments.

This new method is grounded upon that essential quality of the air, its gravity, or pressure: As the column of mercury in the barometer is counterpois'd by a column of air of equal weight; so whatever causes concur to make the air heavier, or lighter, its pressure will be thereby increas'd or diminish'd; and consequently the mercury either rise or fall. Again, the air is more or less condens'd, or expanded, in proportion to the weight, or force, which presses it: Hence it is, that in *England*, *Holland*, the maritime provinces of *France*, and in general in all those countries, which border upon the sea, the mercury stands highest; that the higher you remove from the sea into the midland countries, the lower the mercury will descend; because the air likewise becomes more rarified and lighter; and that upon the tops of the highest mountains it falls lowest; and these heights of the mercury in different places are reciprocally, as the expansions of the air.

From these principles, supported by a competent number of observations, it hath been attempted by several learned men, to derive proper tables, by which the height of any place may be determined, if the altitude of the barometer be given; or the altitude of the barometer determined from the given height of the place; and the expansions of the air likewise settled, as they answer to every inch, or part of an inch in the barometer.

Dr. *Scheuchzer* passes over the first experiment of this kind, which was made in the year 1648 (but a few years after the invention of the *Torricellian* tube was made publick in *France* by F. *Mersenne*) by M. *Perier*, according to the directions of the celebrated M. *Pascal*, upon the high mountain *Puy de Domme*, near *Clermont* in *Auvergne*, the height whereof was thereby determined to 500 *French* toises, or 3000 *Paris* feet, *vide* the appendix to M. *Pascal*'s *traité de l'equilibre des liqueurs*, *Paris* edit. *Anno* 1663, 8vo. p. 177. Nor will the Dr's present purpose admit a particular enumeration of those made some time after, *viz.* in 1661, 1665, 6991, by Mr. *George Sinclair*, professor of philosophy in the university of *Glasgow*, upon the cathedral of that university, upon

upon ſeveral high mountains in *Scotland*, and likewiſe in ſome wells and coal-pits, a particular account of which he inſerted in his *ars magna gravitatis & levitatis, Edit. Rotterod. Anno* 1669. *p.* 129, 132, 134, 144, *ſeq.*

Dr. *Scheuchzer* only obſerves, that theſe experiments of Mr. *Sinclair*, as well as that of M. *Perier*, were intended not ſo much to lay the foundation of a calculation, whereby to determine the different heights of places, as to prove the gravity and preſſure of the air, a problem very much controverted at that time; and to ſhew that the ſame is much more conſiderable in valleys than at the top of mountains; and ſtill greater in proportion at the bottom of wells, mines, *&c.*

But this matter was purſued ſtill farther by the members of the *Royal Academy of Sciences* at *Paris*; particularly, when by order of *Lewis* XIV, they drew that expenſive meridian line a-croſs the whole kingdom of *France*. M. *Mariotte*, a celebrated member of that Academy, was one of the firſt that laid down certain rules for the conſtruction of ſuch tables, as might ſerve to determine both the elevation of places above the level of the ſea from given altitudes of the mercury, and the heights of the air, anſwering to every line of mercury in the barometer, from 28″, where the mercury was ſuppos'd to ſtand at a medium near the ſea. The principles he went upon, and the method he followed, he handled at large, in his *ſeconde eſſaie de la nature de l'air.*

Some time after in 1686, the ingenious Dr. *Halley* ſet about another calculation, which he deriv'd partly from principles agreeing with thoſe of M. *Mariotte*, partly from the ſpecific weight of air and mercury, which were found by experiments to be as 1 to 10800; air being to water as 1 to 800; and water to mercury as 1 to 13 $\frac{1}{2}$, or very near it. If ſo, as the column of mercury in the barometer is counterpois'd by a column of air of equal weight, a cylinder of air of 10800 inches, or 900 foot will be equal to 1 inch of mercury, and 90 feet to $\frac{1}{10}$ of an inch, or 75 to $\frac{1}{12}$ part of it. The height of the air, as it anſwers to one inch of mercury, being thus determined, and the expanſions of the air being reciprocally as the heights of mercury; Dr. *Halley* by help of the *hyperbola* and its aſymptotes, calculated two tables, one ſhewing the altitude to given heights of mercury, the other the heights of mercury at given altitudes. Theſe tables, the firſt that ever were calculated, together with the Dr's whole method of procedure, and an ingenious

attempt of his to discover the true reason of the rise and fall of the mercury upon change of weather, were publish'd in *Phil. Trans.* N° 181. p. 106. and the tables themselves were afterwards re-published, with some observations on them, by Dr. *Desaguliers* in *Phil. Trans.* N° 386.

In 1703, when the meridian line, first begun by M. *Picard* in 1669, afterwards continued in 1683, was farther pursued, several observations of this kind were made, and the heights of several considerable mountains, particularly in the southern parts of *France*, determined as well by trigonometrical, as by barometrical observations. M. *Cassini* the younger took that opportunity to compare these observations with the rules laid down by M. *Mariotte*, vide *Mem. de l'Acad. Royale Ann.* 1705, p. 61. and *seq.* in order to which, and conformable to the said rules, he calculated two tables, one shewing the height of the atmosphere, as it answers to every line of mercury in the barometer, the other determining the height of the atmosphere above the level of the sea at given altitudes of the mercury. But having afterwards upon comparison found, that the observations made in 1703, did not in the main agree with the rules of M. *Mariotte*, and that the heights of places, as they appear'd by those observations, exceeded, generally speaking, the numbers resulting from the tables, made by him according to the said rules, he thought it necessary to calculate two new ones, wherein, it is true, the results are considerably greater than in the tables, framed according to the rules of M. *Mariotte*; insomuch that, for instance, a place, where the mercury falls to 22 inches, rises above the level of the sea according to *Mariotte* 852 toises, or 5112 *Paris* feet; and according to *Cassini* 1158 toises, or 6948 feet, which makes a difference of 1836 *Paris* feet, or 306 toises. Dr. *Desaguliers* in his dissertation concerning the figure of the earth, *Phil. Trans.* N° 386, hath already shewn how far the observations, made by the gentlemen that drew the meridian across the kingdom of *France*, differ from each other; insomuch that there are not two in nine, where the number of toises, said to correspond to the heights of the barometer, agree together; and that consequently the heights of mountains, as determined by those observations, are little to be depended on.

Dr. *John James Scheuchzer*, in his journeys over the mountains of *Switzerland*, as they are more particularly calculated for the improvement of natural philosophy in its several branches, neglected

neglected no opportunity along with his other observations, to make such experiments with the barometer, as might serve to illustrate the qualities of the air, to settle the respective heights of places, and particularly to shew, how much our mountains rise, as well above the level of the sea, as above other neighbouring mountains in *France*, *Italy*, *Spain*, &c. several of these observations are scattered up and down in his writings, particularly his *itinera Alpina* and the several parts of his *Natural history of Switzerland*, which last work was published in *High Dutch*.

It would be too tedious to mention all the experiments he made at different times, and upon different mountains: But Dr. *J. G. Scheuchzer*'s design here requires him to be particular in one experiment, which, for the height, measured both with the line and barometer, is, he thinks, the most considerable that ever was made, and which enabled the former more particularly to examine the two tables, made by *Cassini* the younger, according to the rules of M. *Mariotte*, and the observations made by him and others, when the meridian line was perfected in 1703.

This curious experiment was made *Anno* 1709 at *Pfeffers*, a celebrated mineral spring in the county of *Sargans*, at the foot and top of a mountain, which rises from a small brook, called the *Taminna*, to the height of 714 *Paris* feet, as appeared by letting a line drop down perpendicularly from a tree at top quite to the bottom. At the foot of this mountain, near the *Taminna*, the mercury by repeated experiments was observed at 25″ 9‴ and $\frac{1}{2}$; and at the top it descended to 24″ 11‴ and $\frac{1}{3}$: So that it fell just 10 lines, for 714 feet, which gives about 71 *Paris* feet for each line; if the heights answering to every line were supposed equal.

It is to be noted, that Dr. *J. G. Scheuchzer* has here made use of *Paris* measure, namely toises (o) feet (′) inches (″) and lines (‴). Every toise is reckoned at 6 feet, the foot is divided into 12 inches, and the inch into 12 lines.

The heights of the barometer at the foot and top of the mountain being thus given, the height of it should, according to M. *Mariotte*, be 116° 0′ 8″ 11‴, or 696 *Paris* feet, 8″ 11‴ which falls 17′ 3″ 1‴ short of the true height, and according to *Cassini* 153° 3′ 8″, that is 921 *Paris* feet, 8″, which exceeds the true height by 207 *Paris* feet, 8 inches: Whereby it appears, that the table made according to M. *Mariotte*'s rules is much preferable to that of *Cassini* the younger.

The

The same was likewise confirmed by another experiment made in *June* 1715, on the steeple of the cathedral at *Zurich*. At the foot of the steeple the barometer stood at 26″ 10‴, and at the top 26″ 7‴ and ½; and the height of the steeple was by the line found to be 241 *Paris* feet, 4 inches; which gives very near 69 *Paris* feet for one line, According to M. *Mariotte's* table, the height of the steeple should have been 237 *Paris* feet; according to *Cassini*, 265; and according to the new calculation (of which anon) made pursuant to the above experiments, it comes to 243° 16″ 2‴, or about two foot more than the true height.

It appearing by the experiments made at *Pfeffers*, that from 25″ 9‴ and ⅓, the the barometer descends to 24″ 11‴ and ⅓, that is, just 10 lines, for the height of 714 feet, and the expansions of the air being reciprocally as the heights of mercury; Dr. *John Scheuchzer* undertook, pursuant to these principles, and the properties of the hyperbola, to calculate a new table, after the following method.

As the difference of the logarithms of the two given heights of the barometer 25″ 9‴ and ⅓; and 24″ 11‴ and ⅓, that is, 309 and ⅓, and 299 and ⅓; or 928—898	Is to a foot :	: So is the difference of the logarithms of the height of mercury near the sea, 28″, 1‴, to any lesser height, as for instance 28″, that is 337—336; or 1011—1008	To the height of the atmosphere above the level of the sea, as it answers to 1 line of mercury, is
142717	714	12906	64′ 6″ 9‴

Thus the height of the atmosphere at 28″ appears to be 10° 4′ 6″ 9‴; but according to *Mariotte* it is only 10° 3′, or 63 feet; and *Cassini* supposes it only at 10°, or 60 feet.

In like manner the height of the atmosphere from 28″ to 27″ 11‴ is found to be 64′ 9″ 2‴. According to the same rule, half the height of the atmosphere, that is, the height of the place, where the mercury in the barometer would descend to 14 inches, appears to be 15060′ 3″, or 2510° 0′ 3″. Still upon the same principle the mercury will descend to one line at the height of 133397 *Paris* feet above the level of the sea, which make

make 22232 toises, 5 feet, or 11 *Paris* miles (at 2000 toises a mile) 232 toises, 5 foot. But as in order to determine the whole height of the atmosphere, the logarithm of 1‴ ought to be deducted from the logarithm of 336‴, or 28″; and as that logarithm is 00000, it follows from thence, that beyond the place, where the mercury would descend to 1‴, the air is expanded into an indefinite space.

For the satisfaction of the curious Dr. *J. G. Scheuchzer* adds the tables themselves, to wit those which *Cassini* the younger calculated according to *Mariotte*'s rules, those which he deduced from the observations, made by the Gentlemen of the *Royal Academy of Sciences*, who drew the meridian line, and those which Dr. *John Scheuchzer* calculated from the observation made at *Pfeffers* in 1709.

Tables

TABLES of the Height of the ATMOS-

The fall of mercury in the barometer.		The height of the atmosphere, as it answers to every line in the barometer, according to *Mariotte*.				According to *Cassini*.		According to Dr. *Scheuchzer*.			
"	'''	°	'	"	'''	°	'	°	'	"	'''
0	0	10	3	0	0	10	0	10	4	6	9
	1	10	3	2	6	10	1	10	4	9	2
	2	10	3	4	3	10	2	10	4	11	5
	3	10	3	6	10	10	3	10	5	1	1
	4	10	3	9	1	10	4	10	5	4	1
	5	10	3	11	4	10	5	10	5	6	6
	6	10	4	1	9	11	0	10	5	8	11
	7	10	4	4	1	11	1	10	5	11	3
	8	10	4	6	5	11	2	10	0	1	8
	9	10	4	8	10	11	3	11	0	4	1
	10	10	4	11	2	11	4	11	0	6	6
	11	10	5	1	7	11	5	11	0	8	11
1	0	10	5	4	0	12	0	11	0	11	4
	1	10	5	6	5	12	1	11	1	1	10
	2	10	5	8	10	12	2	11	1	4	4
	3	10	5	11	1	12	3	11	1	6	10
	4	11	0	1	9	12	4	11	1	9	5
	5	11	0	4	2	12	5	11	2	0	0
	6	11	0	6	9	13	0	11	2	2	7
	7	11	0	9	3	13	1	11	2	5	2
	8	11	0	11	10	13	2	11	2	7	9
	9	11	1	2	4	13	3	11	2	10	4
	10	11	1	4	11	13	4	11	3	1	0
	11	11	1	7	7	13	5	11	3	6	4
2	0	11	1	10	2	14	3	11	3	9	0
	1	11	2	0	9	14	1	11	3	11	8
	2	11	2	3	4	14	2	11	4	2	4
	3	11	2	6	0	14	3	11	4	5	0
	4	11	2	8	8	14	4	11	4	7	8
	5	11	2	11	4	14	5	11	4	10	4
	6	11	3	2	1	15	0	11	5	1	0
	7	11	3	4	10	15	1	11	5	3	9
	8	11	3	7	7	15	2	11	5	6	7
	9	11	3	10	4	15	3	11	5	9	5
	10	11	4	1	1	15	4	12	0	0	5
	11	11	4	3	11	15	5	12	0	3	5
3	0	11	4	6	9	16	0	12	0	6	6
	1	11	4	9	6	16	1	12	0	9	8
	2	11	5	0	4	16	2	12	1	0	10
	3	11	5	3	3	16	3	12	1	4	0
	4	11	5	6	2	16	4	12	1	7	2
	5	11	5	9	1	16	5	12	1	10	4
	6	12	0	0	0	17	0	12	2	1	6

PHERE to given Altitudes of *Mercury*.

The height of the atmosphere above the level of the sea, according to *Mariotte*.				According to *Cassini*.		According to Dr. *Scheuchzer*.				Height of the mercury in the barom.	
°	′	″	‴	′	″	°	′	″	‴	″	‴
0	0	0	0	0	0	0	0	0	0	28	0
10	3	2	3	10	1	10	4	9	2	28	11
21	0	6	9	20	3	21	3	8	7	28	10
31	4	1	7	31	0	32	2	9	8	28	9
42	1	10	8	41	4	43	2	1	9	28	8
52	5	10	0	52	3	54	1	8	3	28	7
63	3	11	9	63	3	65	1	5	2	28	6
74	2	3	10	74	4	76	1	4	5	28	5
85	0	10	3	86	0	87	1	6	1	28	4
95	5	7	1	97	3	98	1	10	2	28	3
106	4	6	3	109	1	109	2	4	8	28	2
117	3	7	10	121	0	120	3	1	7	28	1
128	2	11	10	133	0	131	4	0	11	27	0
139	2	6	3	145	1	142	5	2	9	27	11
150	2	3	1	157	3	154	0	7	1	27	10
161	2	2	5	170	0	165	2	1	11	27	9
172	2	4	2	182	4	176	3	11	4	27	8
183	2	8	5	195	3	187	5	11	4	27	7
194	3	3	2	208	3	199	2	1	11	27	6
205	4	0	5	221	4	210	4	7	1	27	5
216	5	0	3	235	0	221	1	2	10	27	4
228	0	2	7	248	3	233	4	2	2	27	3
239	1	7	6	262	1	245	1	3	2	27	2
250	3	3	1	276	0	256	4	9	6	27	1
261	5	1	3	290	0	268	2	6	6	26	0
273	1	2	0	304	1	280	0	6	2	26	11
284	3	5	4	318	3	291	4	8	6	26	10
295	5	11	4	333	0	303	3	1	6	26	9
307	2	8	0	347	4	315	1	9	2	26	8
318	5	7	4	362	3	327	0	7	6	26	7
330	2	9	5	377	3	338	5	8	6	26	6
342	0	2	3	392	4	350	5	0	5	26	5
353	3	9	10	408	0	362	4	7	0	26	4
365	1	8	2	423	3	374	4	4	5	26	3
376	5	9	3	439	1	386	4	4	10	26	2
388	4	1	2	455	0	398	4	8	3	26	1
400	2	7	11	471	0	410	5	2	9	25	0
412	1	5	5	487	1	423	0	0	5	25	11
424	0	5	9	503	3	435	1	1	3	25	10
435	5	9	0	520	0	447	2	5	3	25	9
447	5	3	2	536	4	459	4	0	5	25	8
459	5	0	3	553	3	471	5	10	9	25	7
471	5	0	3	570	3	484	2	0	3	25	6

The fall of mercury in the barometer.		The height of the atmosphere, as it answers to every line in the barometer, according to *Mariotte*.				According to *Cassini*.		According to Dr. *Scheuchzer*.			
"	'''	°	'	"	'''	°	'	°	'	"	'''
	7	12	0	2	11	17	1	12	2	4	8
	8	12	0	5	11	17	2	12	2	8	0
	9	12	0	8	11	17	3	12	2	11	2
	10	12	0	11	11	17	4	12	3	2	4
	11	12	1	2	11	17	5	12	3	5	8
4	0	12	1	6	0	18	0	12	3	8	10
	1	12	1	9	1	18	1	12	4	0	0
	2	12	2	0	2	18	2	12	4	3	2
	3	12	2	3	3	18	3	12	4	6	4
	4	12	2	6	5	18	4	12	4	9	6
	5	12	2	9	7	18	5	12	5	0	8
	6	12	3	0	9	19	0	12	5	3	10
	7	12	3	3	11	19	1	12	5	7	0
	8	12	3	7	2	19	2	12	5	10	2
	9	12	3	10	5	19	3	13	0	1	4
	10	12	4	1	9	19	4	13	0	4	6
	11	12	4	5	0	19	5	13	0	7	8
5	0	12	4	8	4	20	0	13	0	10	10
	1	12	4	11	8	20	1	13	1	2	3
	2	12	5	3	0	20	2	13	1	5	8
	3	12	5	6	5	20	3	13	1	9	1
	4	12	5	9	10	20	4	13	2	0	6
	5	13	0	1	4	20	5	13	2	4	2
	6	13	0	4	9	21	0	13	2	7	10
	7	13	0	8	3	21	1	13	2	10	6
	8	13	0	11	10	21	2	13	3	3	2
	9	13	1	3	4	21	3	13	3	6	10
	10	13	1	6	11	21	4	13	3	10	2
	11	13	1	10	6	21	5	13	4	2	2
6	0	13	2	2	2	22	0	13	4	5	10
	1	13	2	5	10	22	1	13	4	9	6
	2	13	2	9	6	22	2	13	5	1	2
	3	13	3	1	3	22	3	13	5	4	10
	4	13	3	5	2	22	4	13	5	8	6
	5	13	3	8	9	22	5	14	0	0	2
	6	13	4	0	7	23	0	14	0	4	2
	7	13	4	4	5	23	1	14	0	8	3
	8	13	4	8	3	23	2	14	1	0	4
	9	13	5	0	1	23	3	14	1	4	5
	10	13	5	4	0	23	4	14	1	8	6
	11	13	5	8	0	23	5	14	2	0	7
7	0	14	0	0	0	24	0	14	2	4	8
	1	14	0	4	0	24	1	14	2	8	9
		14	0	8	0	24	2	14	3	0	10

The height of the atmosphere above the level of the sea, according to *Mariotte*.				According to *Caffini*.		According to Dr. *Scheuchzer*.				Height of the mercury in the barom.	
°	′	″	‴	′	″	°	′	″	‴	″	‴
483	5	3	2	587	4	496	4	4	11	25	5
495	5	9	1	605	0	509	1	0	11	25	4
508	0	6	0	622	3	521	4	0	1	25	3
520	1	5	11	640	1	534	1	2	5	25	2
532	2	8	10	658	0	546	4	8	1	25	1
544	2	3	10	676	0	559	2	4	11	24	0
556	5	11	11	694	1	572	0	4	11	24	11
569	2	0	1	712	3	584	4	8	1	24	10
581	4	3	4	731	0	597	3	2	5	24	9
594	0	9	9	749	4	610	1	11	11	24	8
606	3	7	4	768	3	623	1	0	7	24	7
619	0	1	1	787	3	636	0	4	5	24	6
631	4	0	0	806	4	648	5	11	5	24	5
644	1	7	2	826	0	661	5	9	7	24	4
656	5	5	7	845	3	674	5	10	11	24	3
669	3	7	4	865	1	688	0	3	5	24	2
682	2	0	4	885	0	701	0	11	1	24	1
695	0	8	8	905	0	714	1	9	11	23	0
707	5	8	4	925	1	727	3	0	2	23	11
720	4	11	4	945	3	740	4	5	10	23	10
733	4	3	9	966	0	754	0	2	11	23	9
746	4	3	7	986	4	767	2	3	5	23	8
759	4	4	11	1007	3	780	4	7	7	23	7
772	4	9	8	1028	3	794	1	3	5	23	6
785	5	5	11	1049	4	807	4	1	11	23	5
799	0	5	9	1071	0	821	1	5	1	23	4
812	1	9	5	1092	3	834	4	11	11	23	3
825	3	4	0	1114	1	848	2	10	5	23	2
836	5	2	6	1136	0	862	1	0	7	23	1
852	1	4	8	1158	0	875	5	6	5	22	0
865	3	10	0	1180	1	889	4	4	11	22	11
879	0	8	0	1202	3	903	3	6	1	22	10
892	3	9	3	1225	0	917	2	10	11	22	9
906	1	2	5	1247	4	931	2	7	5	22	8
919	4	11	2	1270	3	945	2	7	7	22	7
932	2	11	9	1293	3	959	2	11	9	22	6
947	1	4	2	1316	4	973	3	8	0	22	5
961	0	0	5	1340	0	987	4	8	4	22	4
974	5	0	6	1363	3	1002	0	0	9	22	3
988	4	4	6	1387	1	1016	1	9	3	22	2
1002	4	0	6	1411	0	1030	3	9	10	22	1
1016	4	0	6	1435	0	1044	0	2	6	21	0
1030	4	4	6	1459	1	1058	2	11	3	21	11
1044	5	0	6	1483	3	1073	0	0	1	21	10

Observations of a Difference of Sex *in* Misleto; *by Mr.* Edmund Barrel. Phil. Transf. N° 405. p. 547.

MR. *Barrel* being informed that the very learned Dr. *Herman Boerhave* had already told the world of a difference of sex in misleto; and having since that time seen the *historia plantarum*, which is publish'd under *Boerhave's* name, finds that he mentions it in such a manner, as makes Mr. *Barrel* suspect, that he only took his notion from *Tournefort*, and was not fully appriz'd of the true nature of these plants; *ovarium alio à flore loco natum* seems to suppose both flower and ovary to be on the same plant, tho' in distinct places thereof.

Mr. *Barrel* had (from his own sowing of the berries) four thriving plants of misleto growing on one tree in his garden: These being often in his view, gave him the first apprehension of there being any difference of kind, or sex in this shrub: They were not of age to bear flower, or fruit till 1726; when one of them bore a berry or two; and expecting that they should all do so the following year, he frequently examined them, and found that two plants had berries, and two had none: He then examined the misleto on other trees, which had plants upwards of 20 years growth; and he found the method of nature to be thus.

Dr. *Grew* observes, that several plants make a visible preparation in the preceeding year for the flower and fruit of the next season: This is done by misleto. At the latter end of *May*, the male plants put out little knobs, at the joints and tops of their boughs; which at first are not very unlike the young green berries; but they soon appear plainly distinct from them, and being by the latter end of *July* grown as large as the berries, they are then not at all like them; spreading wider upwards, and having three, four, or five buds, at the top of each knob. About *June* the female plant also makes a like preparation, putting out, at the joints and tops of the boughs, knobs which are sharper and shorter than those of the male; with one, or two, but most commonly with three buds, or small points at the top of each knob: Mr. *Barrel* calls them buds, because in their season they open into flowers, both in the male and female plants; all the rest of the knob serving only for foot stalks to the flowers in the one sort, and to both flower and fruit in the other. By the latter end of *August* the berries are grown much larger than the knobs

knobs on the male plants; and from thence till late in *January*, there is little worth remark in either plant; only the berry grows somewhat bigger, and becomes ripe; and the knobs on the male grow more and more yellow: So that one may at that time discern a male from a female plant at a considerable distance. By the 20. of *February* misleto, both male and female, is in bloom. The knobs of the male are open at top, with three, four, or five blossoms, which are very well described (tho' briefly) in *Boerhave*'s *historia plantarum*.

The female plant also flowers now, with a blossom (which *Boerhave* calls the *ovarium*) exactly like the male flower; only that the whole female flower is no bigger than one leaf of the male flower. They both continue in full bloom till the middle of *March*, when the male blossoms begin to wither and drop off. And by the 20th of *March* the young berries begin to shew themselves, swelling forth, one under each female blossom; which often adheres to the top of the berry; and being carried up with it presently withers, and soon falls off again; tho' some continued on till the 12th of *May*, when the berries were of the size of a large pin's head.

This compleated the year's observation. And he thinks it is much to be wondered at, that this plant, which has been the admiration of all ages, should scarce ever find one observer so curious as to follow the changes of it, thro' one whole year's revolution. For, had this been done with any accuracy, it must have been very evident, that one sort of misleto was very different from the other; one sort bearing very small flowers, with berries succeeding them; the other bearing much larger flowers, not succeeded by any berries, the very footstalk of the male falling off with the flower; whereas the footstalk of the female becomes a footstalk to the berry.

It is possible that this difference of sex in the plants of misleto may be of consequence in medicine: To those, therefore, who would make any experiments of the different virtues of these plants, Mr. *Barrel* offers this general observation; *viz.* that there is no time of the year wherein the difference of these two sorts or sexes of misleto is not very easy to be seen and known by the marks abovementioned; and the meanest herb-woman will soon have skill enough to bring the sort she is ordered to procure, there being as great a plenty of the male plants, as there is of the female.

An

An uncommon sinking of the Ground in Kent. Phil. Trans. N° 405. p. 551.

THE sinking of the lands at *Lymne* in *Kent* happened about two years before, and was the consequence of a very wet season; when the waters that had fallen on the up-lands, and were not carried off by drains, soaked into the ground in such quantities, as to form a quick-sand at some considerable depth in the earth (at least this was what was reckoned to have caus'd the phenomenon) which not being able to bear the weight upon it, broke out at the side of the hill, and rais'd its lower parts, letting the brow sink 40 or 50 foot, as the author conjectures.

The ground sunk in one night, and was not perceiv'd by the farmer's family, till they found the change in the morning, by their door-cases not suffering the doors to open. The house was strangely rent by this accident, and had it not been timber built, must have fallen down (as a very strong barn near it did, which was built of stone) for, one great crack of the earth went thro' the middle of it, and split a large kitchen chimney from top to bottom.

a b c d (Fig. 5. Plate X.) represents the profile of the land; *a* the flat land at bottom three or four miles from the sea; *d* the flat land at top, stiff ground and rocky: * The place of the farm at present, which not only sunk down from *d* 40 or 50 foot, but was likewise mov'd somewhat towards *a*; *b* the lower part rais'd to *c*.

Astronomical Observations *at* Peking; *by F.* Ignatius Kögler. Phil. Trans. N° 405. p. 553. *Translated from the* Latin.

	h.	′	″	
Nov. 5. 1724 N. S. The IIII sattellite of *Jup.* was immerged into his shadow	6	9	0	P. M.
Nov. 20. *Jupiter*'s II satellite emerged	6	44	0	Evening
Nov. 30. His I satellite emerged	6	14	0	Evening
Dec. 23. His I satellite emerged	6	19	0	Evening

1725.

March 11. About 9 o'clock in the morning, the sun shining between thin clouds, and surrounded with a colour'd halo, form'd therein two very bright *parhelia* to the right and left; this appearance lasted for half an hour.

May 10. At 4 o'clock in the morning *Jupiter* was distant from φ of *Aquarius* 9′ 5″ to the west; the distance to be computed from the centre.

May

May 11. At 4 o'clock in the morning *Jupiter* being now paſt φ, his centre was diſtant therefrom 1′ 10″ to the north-eaſt: At 5, the diſtance of the centres was 1′ 36″.

May 12. At 4 o'clock in the morning *Jupiter* was diſtant from φ 10′ 10″.

May 13. At 4 o'clock in the morning *Jupiter* was diſtant from φ 19′ 50″.

	h.	′	″	
June 23. *Jup.* III ſat. enters into his ſhadow	2	29	0	
July 9. The I ſat. immerges into his ſhadow	2	55	30	
Aug. 9. The I ſatellite immerges	11	27	0	Evening
Aug. 31. The I and II ſatellites in their neareſt conjunction almoſt coaleſce into one: It could not be diſtinguiſhed which firſt immerged into the planet's ſhadow	11	45	0	
Sep. 19. The I ſatellite emerges	6	51	30	Evening
Oct. 2. The I ſatellite emerges	10	45	0	Evening
Oct. 10. The I ſatellite emerged	12	42	0	Morning
Oct. 11. Emerſion of the I ſatellite	7	9	0	Evening
Oct. 15. The III emerges from behind *Jup.*	6	46	0	Evening
Then it diſappear'd in *Jup.* ſhadow	7	4		
At length it emerged	10	20		
Oct. 19. The emerſion of the I ſatellite	9	9		Evening
Oct. 20. Emerſion of the II ſatellite	9	6		Evening
Oct. 25. Emerſion of the I ſatellite	11	6		Evening
Oct. 27. Emerſion of the II ſatellite	11	45	30	Evening
Nov. 3. Emerſion of the I ſatellite	7	27	40	Evening
Nov. 19. Emerſion of the I ſatellite	5	42	30	Evening
Nov. 20. The III ſat. began to emerge	6	26	30	Evening

A total Eclipſe *of the* Moon, Oct. 22*d at Midnight.*

	h.	′	″
The beginning of the true ſhadow very near the node	0	49	0
The edge of the ſhadow touch'd *Grimaldus*	0	51	0
Ariſtarchus	0	55	30
Keplerus	0	59	0
Mare humorum	1	2	0
Gaſſendus	0	3	0
Sinus Irid. & Morin.	0	5	0

	h.	'	
Copernicus	0	6	30
Bullialdus	0	9	30
Eratosthenes	0	11	0
Plato	0	54	0
Tycho	0	59	0
Aratus: Tycho entirely covered	0	59	30
Manilius	0	22	0
The edge of the ſhadow touches *Menelaus*	0	24	30
S. *Dionyſius*	0	27	0
Pliny	0	29	0
Poſſidonius	0	31	0
S. *Catharina*	0	32	0
S. *Theophilus* & *Cenſorinus*	0	35	30
Palus ſomni	0	37	30
Proclus	0	39	30
Tachenius and the eaſtern ſhore of *Mare Criſium*	0	40	0
The extremity of the weſtern ſhore of *Mare Criſium*	0	43	0
Langrenius	0	44	0
The total immerſion near the weſtern node	0	46	30
Recovery of the firſt light at the eaſtern node	3	27	30
The eaſtern edge of *Grimaldus* emerges	0	30	30
The weſtern edge of *Grimaldus* emerges	0	31	30
Galilæus emerges	0	32	30
Ariſtarchus	0	36	0
Keplerus	0	39	0
The eaſtern ſhore of *Mare humorum*	0	39	30
Gaſſendus	0	43	0
Plato	0	49	30
Timocharis	3	51	0
Tycho entirely emerged	0	54	30
Sinus æſtuum entirely emerged	0	59	0
Manilius emerges	4	3	0
Menelaus	0	6	0
Poſſidonius and *Endymion*	0	9	0
Plinius	0	10	30
Cenſorinus	0	15	30
Palus ſomni	0	16	30
The eaſtern ſhore of *Mare Criſium*	0	19	0
The extremity of the weſtern ſhore	0	22	0
Langrenius	0	24	30
The end of the eclipſe about	0	26	0
The weſtern node	0	0	0

The

The clock was corrected by culminations of *Palilicium* and some stars of *Orion*. The apparent diameter of the moon immediately before and after the eclipse was 32′ 30″ nearly.

Observations made at Ingolstad *in* 1726; *by the* Jesuits. Phil. Trans. N° 405. p. 556. *Translated from the* Latin.

	h.	′	″
JAN. 6. *Jupiter*'s satellite emerged, with a 14 foot telescope of S. *Campani*'s	6	40	30
Jan. 19. *Mars* shining thro' vapours stood at the bright limb of the moon	6	52	e
Now he was entirely immerged	6	54	0
The centre of *Mars* emerges out of the obscure limb of the moon	7	54	25
Mars entirely emerged	7	54	35

The transit of *Mars* was in a line drawn from the centre of *Grimaldus* thro' the northern extremity of *Langrenus*. Hence, having a regard to the moon's vibration, is gather'd the least distance of the centres, *viz.* 2′ 30″; *Mars* being more southerly: The apparent semi-diameter of the moon at 9 o'clock was 16′ 55″. The observation was made with a 10 and 12 foot telescope.

	h.	′	″
June 9. Immersion of the innermost satellite of *Jupiter* with a 23 foot telescope	15	4	20
July. 17. Immersion of the same; doubtful	13	24	45
July 20. Immersion of the II satellite with a nine foot telescope	15	16	40
August 1. *Mars* disappear'd into the obscure limb of the moon	5	25	17
The first emersion of *Mars* at *Zoroaster*; the center of which *macula* at that time possess'd the lucid limb of the moon	6	1	53
The total immersion of *Mars* with a 12, 14 and 16 foot telescope	6	1	59

The apparent diameter of the moon at half an hour after 7 was 32′ 47″.

	h.	′	″
August 2. Immersion of *Jupiter*'s innermost satellite with a 12 foot telescope	11	41	20
August 14. Immersion of the II satellite with the same telescope	12	25	6

	h.	′
Auguſt 25. Immerſion of the innermoſt ſatellite with a 23 foot teleſcope	11	56
Auguſt 26. The III ſat. begins to emerge	11	43
Sept. 1. Immerſion of the innermoſt ſatellite with a 23 foot teleſcope	13	51
Sept. 2. The III ſat. entirely emerged	13	17
The ſame day the firſt emerſion of the III ſatellite with a 10 foot teleſcope	15	45
Sept. 9. The emerſion of the II ſatellite with a 14 foot teleſcope about	9	40
The ſame day immerſion of the innermoſt ſatellite with a 23 foot teleſcope	15	50
The ſame day the innermoſt ſatellite entirely immerged about	17	20
Sept. 10. An immerſion of the innermoſt ſat.	10	19

These two eclipſes were obſerv'd at *Biturgum*, the reſidence of the *Ingolſtad* College; which has elſewhere been determined to be 1′ 40″ to the eaſt of the meridian of *Ingolſtad*.

	h.	′
Sept. 10. Immerſion of the innermoſt ſatellite with a 14 foot teleſcope	10	17
Sept. 16. Immerſion of the innermoſt ſatellite with the ſame teleſcope		39

A ſolar Eclipſe *obſerv'd at* Ingolſtad Sept. 25.

		h.	′
The ſun's image being receiv'd by a helioſcope into a darkened room, gives the beginning of the eclipſe about 46° and ½ from the nadir to the north		5	17
At an obſervatory about 100 paces diſtant from the former place, he was obſerv'd with a 12 and 16 foot teleſcope to be eclipſed $\frac{1}{10}$ of a digit		5	19
The centre of the *macula* next the ſun's limb is immerged		5	23
The centre of the remarkable *macula*		5	24
The centre of the third *macula*		5	26
II dig. eclips'd from the nadir to the N.	39°	5	30
III dig.	35 ½	5	37
IV dig.	27	5	43
Clouds intercepted the ſun, when eclipſed about IV dig. and a half		5	49

The phaſes meaſur'd by the micrometer.

				h.	′	″
I dig.	—	—	—	5	22	30
II	—	—	—		30	50
III	—	—	—		37	54
IV	—	—	—		44	30
V dig. 33′	—	—	—		47	30

The ſun's ſemi-diameter, ſeveral times meaſur'd by the micrometer was exactly 16′.

On the ſun's diſk there appear'd ſeveral *maculæ* different from the four taken notice of in the immerſion; but they were ſmaller than that their immerſion could be diſcern'd thro' the vapours that obſcur'd the ſun.

An Account of a Machine *for meaſuring any Depth in the* Sea, *with great Expedition and Certainty, contriv'd by Mr.* Stephen Hales *and Dr.* Deſaguliers. Phil. Tranſ. N° 405. p. 559.

THERE have been ſeveral machines contriv'd for meaſuring the different depths of the ſea; eſpecially ſuch as could not be determined by the lead and line; but as theſe machines conſiſted of two bodies (the one ſpecifically lighter, and the other ſpecifically heavier than water) joined together in ſuch a manner, that as ſoon as the heavy one came to the bottom, the lighter ſhould get looſe from it, and emerge; and the depth was to be eſtimated by the time of the fall of the compound body from the top to the bottom of the water, together with the time of the emerſion of the lighter body, reckoned from the diſappearing of the machine, till the emergent body were ſeen again, no certain conſequence could be drawn from ſo precarious and complex an experiment.

For, even in ſtill water, and in the ſame place, the time will hardly be the ſame in two experiments; much leſs will this machine anſwer in the ſea, on account of waves and currents, and many other hindrances.

But as the preſſure of fluids in all directions is always the ſame at the ſame depth; a gage, which exactly diſcovers what the preſſure is at the bottom of the ſea, will ſhew what the true depth of the ſea in that place is, whether the time of the deſcent of the machine be but a minute or two, or 20 times as long.

Mr. *Hales* in his *vegetable statics* describes his gage for estimating the pressures, made in opaque vessels; where honey, being pour'd over the surface of mercury in an open vessel, rises upon the surface of the mercury, as it is pressed up into a tube, whose lower orifice is immersed into the honey and mercury, and whose top is hermetically seal'd. Now as by the pressure the air in the tube is condens'd, and the mercury rises; so the mercury comes down again when the pressure is taken off, and would leave no mark of the height, to which it had risen: But the honey (or treacle, which does better) which is upon the mercury, sticking to the inside of the tube, leaves a mark, which shews the height to which the mercury had risen; and consequently shews what was the greatest pressure.

Dr. *Desagulier*'s contrivance, therefore, is a machine, which will carry down Mr. *Hales*'s gage to the bottom of the sea, and immediately bring it up again, as represented in Fig. 6. Plate X.

A B is the gage-bottle; F *f* the gage tube cemented to the brass cap of the bottle at G, with its open end *f* immersed in the mercury C, which by the pressure of 32 feet of water is carried up to *d* with a little treacle or honey *d* upon it, raised up from D, a small thickness of treacle poured on the mercury.

When the pressure of water is from a depth of 64 foot, the mercury and treacle rise up to E, $\frac{2}{3}$ of the height of the tube; and so higher proportionably to the depth.

N. B. A scale may be mark'd on the tube with a diamond.

K represents a weight hanging by its shank L in a socket *m*, fixed to the ring M B, cemented at the bottom of the bottle. When the hole L of the shank is shov'd up to *m*, the catch *l* of the spring S holds it from falling out of the socket, whilst the machine is descending. But as soon as K touches the ground at the bottom of the sea, the hole L rising, the catch flies back and lets go the weight, as represented in the Fig. Then the empty glass-ball I (which at sea may be a hog's bladder) rises up to the surface of the water with the machine; in which observing how high the inside of the tube is daubed; the pressure, and consequently, the depth of the sea is known.

H G represents a brass tube to guard the top of the gage-tube.

There are holes at F, G, and E to let the water pass freely every way.

To confirm the use of this sea gage, Dr. *Desaguliers* made an experiment in the following manner. Having poured some quicksilver into the bottle of the gage, he poured treacle thereon

on to the depth of half an inch, then ſcrewed on the braſs cap of the bottle, to which the glaſs gage-tube was cemented; by which means the open end of the tube was brought under the ſurface of the mercury, the ſeal'd end being upwards. The machine, thus fitted, was immerſed in a cylindric veſſel of water, which with a plate at top was preſſed between two columns, in ſuch a manner, that air might be condens'd over the water without eſcaping: Then having forced in ſo much air with a ſyringe, as to lay on a preſſure, equal to what would be in a depth of 40 foot water, he opened the cock of the upper plate, let out the air; and upon taking out the machine, it appeared how high the quickſilver had riſen in the gage-tube, by the greaſy mark which the treacle left within.

Two uncommon Caſes of Tumours *of the* Abdomen; *by Dr.* Rutty. Phil. Tranſ. N° 405. p. 562.

THE firſt caſe is that of a woman of *Strasburgh*, 32 years of age, whoſe belly, after an immature and haſty labour, grew gradually for 10 years together. During the whole time of geſtation, ſhe complained of ſcarcely any other ſymptom than the weight, and heavineſs of her belly; only now and then of a tenſe pain, and a difficulty in reſpiration: She affirmed, nevertheleſs, that *flatus*'s would ſometimes be diſcharged from the *pudenda*; and the more they were ſo, the leſs uneaſineſs ſhe perceived. The *menſtrua* were regular as to time; but as to quantity ſhe did not explain herſelf: But in the latter months, towards her death, ſhe grew plainly cachectic: Her countenance was cadaverous; her breaſt, and upper limbs perfectly emaciated; her feet oedematous; and the belly much more turgid, and prominent than before; ſo that at length ſhe breathed with the utmoſt difficulty; and upon taking any nouriſhment, complained of a great ſtraitneſs in her cheſt.

Upon opening the *abdomen*, two days after her death, ſome water flowed out, of a wheyiſh colour, tho' in what quantity was not taken notice of: But upon dividing the *uterus*, a plentiful quantity of a bloody liquor iſſued therefrom, together with 72 *molæ* of different figures, and ſolidity; and chiefly of a black colour: One *mola* only adhered to the lower part of the right ſide of the *uterus*, contiguous to its internal orifice. Theſe ſolid ſubſtances weigh'd 64 ounces; and the liquor fill'd 15 old meaſures of *Alſace*: So that taken together the whole weigh'd 80 pounds apothecary weight. The ſkin of the *abdomen*

men was very thin, and almoſt tranſparent; the navel was quite obliterated; the fat almoſt entirely conſumed; the muſcles pale, flaccid, and likewiſe very thin; and the *peritonæum* in ſome places ſo ſtrongly adhered to the *uterus*, that it could not, without the utmoſt difficulty, be torn from it. The body of the *uterus*, which is naturally thick, was extenuated to the ſame degree of rarity and tranſparency with that of the *cutis* of the *abdomen*, and of a ſurpriſing capacity. The liver appeared pale, and ſo flaccid, that it might be eaſily rubbed to pieces. The height of the belly from the *vertebræ* of the loins to the navel meaſured two feet and ½; and its circumference at the waiſt four feet, two inches and ½; tho' the woman was naturally of a ſmall ſize and ſtature.

The ſecond caſe is that of a maid ſervant in the ſame city, 23 years of age; whoſe belly, from a ſuppreſſion of the *menſtrua*, grew ſlowly for three years, without any other notable diſorder: Till upon an accidental fall, it increaſed ſo much in ſix days, as to obliterate the navel; and not being capable of a farther diſtenſion, part of the matter which cauſed the tumefaction, flowed down to the legs, and ſwelled them in like manner; which brought on a difficulty of breathing, a ſmall frequent and uneaſy pulſe, with an entire loſs of appetite. But what was more remarkable the *ſyſtole* and *diaſtole* of the heart were plainly felt under the left clavicle: The heart being, upon diſſection, found thruſt up to that part of the *thorax*. The 14th day from the fall, a *diarrhœa* came on, which killed her in a few days.

Upon making a ſmall inciſion in the right *hypocondrium*, there guſhed out from the cavity a liquor, in colour, conſiſtence and froth, reſembling well boiled beer; which, upon enlarging the inciſion, was followed by a fetid purulent matter, with entire portions of the putrified caul; which matter filled 56 *Strasburgh* pints. Upon this the belly ſubſided; but a large ſolid ſubſtance ſtill remained under the containing parts of the *abdomen*.

Upon opening, therefore, the whole cavity, there was found under the left groin a conſiderable tumour, nouriſhed by its proper veſſels, and every where fixed to the circumjacent membranes; which, when freed, weighed ſix pounds common weight. This tumour proved a congeries of incyſtated *abſceſſes* (wrapt up in one common covering) of different ſizes; the largeſt as big as a man's two fiſts; the ſmalleſt of the ſize of an egg; and each of a different ſort of ſubſtance: Beſides which

which was a great number of *hydatides*. The *peritonæum* was as thick as the *cutis*; the caul almost entirely consumed; the stomach in a natural state, but quite empty; the guts livid, very thick and exceedingly inflated; and moreover preternaturally connected to each other by peculiar membranes. The liver strongly adhered to the right *hypocondrium*, and its coat parted from its *parenchyma* almost spontaneously. The left kidney very nearly equalled the spleen in bulk, and the *pancreas* was as hard as a cartilage; but the *uterus* and bladder were found in a natural state. The cavity of the *thorax* was much smaller than usual, from the contents of the *abdomen* pressing up the diaphragm into it; in which cavity there was also found the same sort of bloody putrid liquor, as likewise in the *pericardium*. The right ventricle of the heart was preternaturally soft and flaccid, and when opened, it was found lin'd with *hydatides*. The upper parts of the body were emaciated; and the lower were much tumified by the water contained therein.

The Culture and Management of Saffron *in* England; *by Dr.* James Douglas. Phil. Trans. N° 405. p. 566.

AS saffron grows at present very plentifully in *Cambridgeshire*, and has formerly grown in several other counties of *England*, the method of culture does not, Dr. *Douglas* believes, vary much in any of them; and therefore he has judged it sufficient to set down here the observations, which he employed proper persons, in different seasons, to make in the years 1723, 1724, 1725, and 1728 up and down in all that large tract of ground, that lies between *Saffron-Walden* and *Cambridge*, in a circle of about 10 miles diameter. In that country saffron has been longest cultivated; and therefore, it may reasonably be expected, that the inhabitants thereof are more thoroughly acquainted with it than they are any where else.

The Dr. begins with the choice and preparation of the ground. The greatest part of the tract already mentioned is an open level country with few inclosures; and it is the custom there, as in most other places, to crop two years, and let the land lie fallow the third: Saffron is always planted on fallow ground; and all other things being alike, they prefer that which has borne barley the year before.

The saffron grounds are seldom above three acres, or less than one; and in choosing them, the principal thing they have regard

gard to is, that they be well expoſed, the ſoil not poor, nor very ſtiff clay, but a temperate dry mold, ſuch as commonl lies upon chalk, and is of a hazel colour; tho' if every thi elſe anſwer, the colour of the mold is pretty much neglected.

The ground being made choice of about *Lady-day* or t beginning of *April*, it muſt be carefully plowed; the furro being drawn much cloſer together and deeper, if the ſoil wi allow it, than is done for any kind of corn; and according the charge is greater.

About five weeks after, or during any time in the month *May*, they lay between 20 and 30 loads of dung upon eac acre; and having ſpread it very carefully, they plow it in before: The ſhorteſt rotten dung is the beſt; and the farme who have the conveniencies of making it, ſpare no pains make it good, being ſure of a proportionable price for i About *Midſummer* they plow a third time; and between eve 16 foot and ½, or pole in breadth, they leave a broad furro or trench, which ſerves both for a boundary to the ſeveral pa cels (when there are ſeveral proprietors to one incloſure) and throw the weeds in at the proper ſeaſon.

To this head likewiſe belongs the fencing of the ground; b cauſe moſt commonly, that is, tho' not always, done before the plant. The fences conſiſt of what they call dead hedges, hurdles to keep out not only cattle of all ſorts, but eſpeciall hares; which would otherwiſe feed on the ſaffron leaves durin the winter.

As to the weather it need only be obſerved, that the hotte ſummers are certainly the beſt; and if accompanied with gent ſhowers from time to time, they can hardly miſs of a plentif rich crop, if the extreme cold, ſnow, or rain of the preceedin winter have not prejudic'd the heads.

The next general part of the culture of ſaffron, is plantin or ſetting the roots; the only inſtrument for which is a narro ſpade, commonly called a *ſpit-ſhovel*.

The time of planting is commonly in *July*, a little ſoone or later according as the weather anſwers. The method is thu One man with his ſpit-ſhovel raiſes between three and fou inches of earth, and throws it before him about ſix or mo inches; two perſons, generally women, following him wit heads, place them in the fartheſt edge of the trench he make at three inches diſtance from one another, or thereabouts. A ſoon as the digger, or ſpitter has gone once the breadth of th ridge, he begins again at the other ſide, and digging as befo

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covers the roots last set; and makes the same room for the setters to place a new row, at the same distance from the first, as they are from one another. Thus they go on, till a whole ridge, containing commonly one rood, is planted; and the only nicety in digging is to leave some part of the first *stratum* of earth untouched to lie under the root; and in setting, to place the roots directly upon their bottoms. But it must be observed in this place, that formerly when roots were very dear, they did not plant them so thick, as they do now; and that they always have some regard to the size of the roots, placing the largest at a greater distance than the small ones.

The quantity of roots planted in an acre is generally about 16 quarters, or 128 bushels, which, according to the distances left between them, as before assigned; and supposing them all to be an inch in diameter one with another, ought to amount to 39:040 in number.

From the time that the roots are planted, till about the beginning of *Sept.* or sometimes later, there is no more labour about them; but as they then begin to spire, and are ready to shew themselves above ground, which is known by digging a few out of the earth, the ground must be carefully pared with a sharp hough, and the weeds, &c. raked into the furrows; because otherwise they would hinder the growth of the plants.

In some time after, appear the saffron flowers; and this leads to the third branch of the present method. The flowers are gathered as well before, as after they are full blown; and the most proper time for this is early in the morning. The proprietors of the saffron get together a sufficient number of hands, who place themselves in different parts of the field, pull off the whole flowers, and throw them by handfulls into a basket; and so continue till all the flowers be gathered, which commonly happens about 10 or 11 o'clock.

Having then carried home all they have got, they immediately spread them upon a large table, and placing themselves round it, they fall to picking out the *filamenta styli* or chives, and together with them a pretty long portion of the *stylus* itself, or string to which they are joined.

The rest of the flowers they throw away as useless. The next morning they return into the field again, whether it be wet or dry weather; and so on daily, even on *Sundays*, till the whole crop be gathered.

The chives being all pick'd out of the flowers, the next labour is to dry them on the kiln. The kiln is built upon a

thick plank (that it may be moveable from place to place) sup-ported by four short legs. The outside consists of eight pieces of wood, about three inches thick, joined in form of a quadrangular frame, about 12 inches square at bottom on the inside, and 22 inches at top, which is likewise equal to the perpendicular height of it. On the foreside is left a hole about eight inches square, and four inches above the plank, thro' which the fire is put in. Over all the rest, laths are laid pretty close to one another, and nail'd to the abovementioned frame, and then are plaistered over on both sides, as is also the plank at bottom very thick, to serve for a hearth. Over the mouth, or widest part goes a hair cloth fixed to two sides of the kiln, and likewise to two rollers, or moveable pieces of wood, which are turned by wedges or screws, in order to stretch the cloth. Instead of the hair-cloth many now use a net-work of iron wire, with which it is observed, that the saffron dries sooner, and with a less quantity of fuel; but the difficulty of preserving the saffron from burning, makes the hair cloth be preferred by the nicest judges in drying.

The kiln is placed in a light part of the house; and they begin by laying five or six sheets of white paper on the hair cloth, upon which they spread the wet saffron, between two and three inches thick. This they cover with other sheets of paper, and over these lay a coarse blanket five or six times doubled, or instead thereof a canvas pillow filled with straw; and after the fire has been lighted for some time, the whole is covered with a board, having a large weight thereon.

At first they give it a pretty strong heat, to make the chives sweat, as they express it; and in this, if they do not use a great deal of care, they are in danger of scorching, and so of spoiling all that is on the kiln.

When it has been thus dried for about an hour, they take off the board, blanket, and upper papers, and take the saffron off from that which lies next it, raising at the same time the edges of the cake with a knife. Then laying on the papers again, they slide in another board between the hair cloth and under papers, and turn both papers and saffron upside down, afterwards covering them as above.

This same degree of heat is continued for an hour longer; then they look to the cake again, free it from the papers, and turn it; then they cover it, and lay on the weight as before. If nothing happen amiss during these first two hours, they reckon the danger to be over; for, they have nothing

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more to do, but to keep a gentle fire, and turn their cake every half hour, till it be thoroughly dried; for doing which, as it ought, there are required full 24 hours.

In drying the large plump chives they uſe nothing; but towards the latter end of the crop, when theſe come to be ſmaller, they ſprinkle the cake with a little ſmall beer to make it ſweat, as it ought; and they now begin to think, that uſing two linen cloths next the cake, inſtead of the two innermoſt papers, may be of ſome advantage in drying; but this practice is hitherto followed but by few.

Their fire may be made of any kind of fuel; but that which ſmoaks the leaſt is beſt, and charcoal for that reaſon is preferred to any other.

What quantity of ſaffron a firſt crop will produce is very uncertain: Sometimes five or ſix pounds of wet chives are got from one rood; ſometimes not above one or two; and ſometimes not enough to make it worth while to gather and dry it. But this is always to be obſerved, that about five pounds of wet ſaffron go to make one pound of dry, for the firſt three weeks of the crop, and ſix pounds during the laſt week; and now the heads are planted very thick, two pounds of dried ſaffron may at a medium, be allowed to an acre for a firſt crop, and 24 pounds for the two remaining, the third being conſiderably larger than the ſecond.

In order to obtain theſe, there is only a repetition to be made every year of the labour of houghing, gathering, picking, and drying in the ſame manner, as before ſet down, without the addition of any thing new; only that they let cattle into the fields, after the leaves are decayed, to feed upon the weeds; or perhaps they mow them for the ſame uſe.

About *Midſummer* after the third crop is gathered, the roots muſt all be taken up, and tranſplanted: The management requiſite for which is the fourth thing to be treated of.

To take up the ſaffron heads, or break up the ground, as they call it; they ſometimes plow it, and ſometimes uſe a forked kind of hough, called a pattock, and then the ground is harrowed once or twice over: During all which time of plowing, or digging and harrowing, 15 or more people will find work enough to follow and gather the heads, as they are turned up.

They are next to be carried to the houſe in ſacks, and there to be cleaned or raſed: This labour conſiſts in clearing the roots thoroughly from earth, and from the remains of old roots

old *involucra* and excrescencies; and they become fit to be planted in new ground immediately, or to be kept for some time without danger of spoiling.

The quantity of roots, taken up in proportion to those that were planted is uncertain; but at a medium it may be affirmed, that allowing for all the accidents that happen to them in the ground, and in breaking up, there may be had from each acre 24 quarters of clean roots, all fit to be replanted. The Proprietors chuse for their own use, the largest, plumpest, and fattest roots, but above all they reject the oblong pointed ones, which they call *spickets* or *spickards*; for, very small, round, or flat roots, are sometimes observed to flower.

This is the whole process of the culture of Saffron in the county above-mentioned. And now as to the charges and profits, which may be supposed one year with another to attend this branch of agriculture, the Dr. has drawn up the following computation from one acre of ground, according to the price of labour in this county.

	l.	s.	d.
Rent for three Years — —	3	0	0
Plowing three times — —	0	18	0
Dunging — —	3	12	0
Hedging — —	1	16	0
Spitting and setting the heads —	1	12	0
Weeding or paring the ground —	1	4	0
Gathering and picking the flowers —	1	10	0
Drying the flowers — —	1	6	0
Instruments of labour for three years, with the kiln, about —	1	10	0
Plowing the ground once, and harrowing twice	0	12	0
Gathering the Saffron heads —	1	0	0
Rasing the heads — —	1	12	0
Total Charge	23	12	0

This calculation is made upon the supposition, that an acre of ground yields 26 pounds of neat Saffron in three years, which the Doctor stated only as a mean quantity between the greatest and least, and therefore the price of Saffron must be adjusted accordingly, which he thinks cannot be better done than by fixing it at 30 s. per pound; since in very plentiful years it is sold for 20; and is sometimes worth between 3 and 4 pounds. At this rate 26 pounds of Saffron are worth 39 pounds; and the neat profits of an acre of ground producing Saffron will in 3 years amount to 15 pounds 13 shillings; or to about 5 pounds 4 shillings.

shillings yearly. This the Dr. says, may be reckoned the neat profit of an acre of saffron, supposing that all the labour were to be hir'd for ready money: But as the planter and his family do a considerable part of the work themselves, some of this expence is sav'd; that is, by planting saffron, he not only may reasonably expect to clear about five pounds yearly per acre; but also to maintain himself and family for some part of each year: And it is upon this supposition only, that the result of other computations, which have been made of the profits of saffron, can be said to have any tolerable degree of exactness; but the calculations themselves are, undoubtedly very unaccurate.

The Dr. has said nothing here concerning the charge in buying, or profits in selling the saffron heads; because in any large tract of ground, these must at length always balance each other; while the quantity of ground planted yearly continues the same, which has been pretty much the case for several years past.

Remarks on the Height of Mountains *in general, and of those in* Switzerland *in particular; by Dr.* J. G. Scheuchzer Phil. Transf. N° 406. p. 577.

IN *Phil. Transf.* N° 405. p. 537, Dr. J. G. *Scheuchzer* took notice that *Dicæarchus* found mount *Pelius* in *Thessaly* to be 1250 paces high, which make 6250 *Roman*, or 6822 *Paris* feet; a height which we may well pronounce too great even for the absolute height of mount *Pelius*, that is, its rise above the level of the sea. Conformable to the determination of *Dicæarchus*, the Dr. mentioned, that *Plutarch* fixes the height of the highest mountains, and the greatest depth of the sea to 10 stadia; and *Cleomedes* affirms, that they cannot exceed 16 stadia. The celebrated *Galilæus de Galilæis* is one of the most modest among the modern writers on this head: For, he affirms (*Nuntius siderius* p. 14.) that the highest mountains do not rise above a mile, or eight stadia, or 5000 old *Roman Vespasian* feet, which make 5458 *Paris* feet above the level of the sea; and which we shall find anon to agree pretty well with some of the highest mountains in *France*; and probably with those in *Italy*. *Kepler* went rather too far *Astronom. Optic. p.* 129, 135, & *Epitom. Astronom. lib.* 1. *p.* 26, when he assigned the mountains of *Rhoetia* (thought the highest in *Switzerland*) a height of 26 stadia, or 10000 old *Roman Vespasian* feet, which make 10916 *Paris*

Paris feet. The opinions of ſome other ancient and moder[n] geographers, and mathematicians will better appear by th[e] annexed table.

A table ſhewing the height of mountains according to ſever[al] ancient and modern authors.

	Stad.	Old Ro. Veſpaſ. feet.	Paris feet.
*Strabo (lib.*II *Geo.)* affirms, that the higheſt mountains, which he calls *Petra Sogdiana*, is	30	18750	204
Pererius (lib. XII *in Geneſin)* determines the higheſt mountains to	32	20000	218
Leo Bapt. Albertus (Architect. lib. XI *cap.* 1.*)* to		22500	236
Ath. Kircher (Ars mag. luc. & Umbr. Par. II *probl.* 5.*)* brings them to	43	26875	293
Eromond (lib I, *meteor cap.* 2. *Art.* 1.*)*	64	40000	436
Gilbertus de magnete lib. IV. *cap.* 1.	128	80000	873
Pliny (lib. III. *cap.* 64.*)* according to *Fortunius Licetus*'s explanation *(de Lunæ luce ſubobſcurâ lib.* II. *p.* 306.*)* to	400	250000	2729
Ricciolus (Geog. lib. VI.*)* is of opinion, in purſuance of what he imagines to have demonſtrated of the mountains *Athos* and *Caucaſus*, that poſſibly there may be mountains of	512	320000	3493

Now in oppoſition to this table, wherein the heights mu[ſt] needs, upon firſt view, appear romantic and unnatural; [let] us conſider the height of ſuch mountains, as have been me[a]ſured, either by trigonometrical or barometrical obſer[va]tions.

In *England* the height of *Snowdon-hill*, one of the high[eſt] mountains in *Wales*, was meaſured trigonometrically [by] Mr. *Caſwell* of *Oxford*, and found to be 1240 yards, or 37[20] *Engliſh* feet, which make 3488 *Paris* feet. At the top [of] this mountain the mercury ſubſided to 25″ 6‴, which bei[ng] reduced to *Paris* meaſure, make juſt 24″. Now in th[e] abov[e]

above tables, the height of the place where the mercury subsides to 24″ is, according to *Mariotte*, 544 toises, 2 foot, or 3266 foot above the level of the sea; according to *Cassini* 676 toises, or 4056 feet, and according to Dr. *John Scheuchzer*'s calculation, 559° 2′, or 3356′: So that *Mariotte* comes 222 feet short of its height, as it was determined trigonometrically; Dr. *Scheuchzer* but 132′; but *Cassini* exceeds this height by 568 feet; which again confirms as was shewn in a former transaction, that *Mariotte*'s table is preferrable to that of *Cassini*, tho' pretended to have been corrected upon the former, and that of Dr. *Scheuchzer* an improvement upon both. According to the observations made by Dr. *Halley May* 26, 1697, the mercury stood at the top of *Snowdon-hill* at 26″ 1‴ *English*; which, if reduc'd as above, would give the height of the mountain somewhat less.

In *France*, when the meridian line, first begun in 1669, was continued in 1703; the heights of several mountains, particularly, in the south of *France*, were determined trigonometrically by the members of the *Royal Academy of Sciences*: And Dr. *Scheuchzer* finds up and down in their memoirs the heights of the following.

	Height in Toises.	Feet.
Mount *Clairet* in *Provence*	277 or	1662
La Massane in *Roussillon*	397	2382
The same according to another observation	408	2448
Bugarach a mountain in *Languedoc*	648	3888

Mountains in *Avergne*

Le Puy de Domme, near *Clermont*	810	4860
La Courlande	838	5028
La Coste	851	5106
Le Puy de Violent	853	5118
Le Cantal	984	5904
Le Mont d'or	1030	6180

In the county of *Avignon*.

Le Mont Ventoux	1036	6215

Pyrenean

Pyrenean mountains.

S. Barthelemy dans le Paix de Foix	1185	711
La Montagne de Mousset	1258	7548
Le Canigon	1440	8640

Before the Dr. proceeds farther, he observes that the heights of these mountains, in the main, seem rather too great. But this is easily accounted for, as they were measured trigonometrically, it having been observ'd that the refraction of the air, gives the height greater than they actually are. But what confirms it still more is, that according to the above tables, the numbers which answer to the heights of the mercury, as they were observ'd at the top of some of those mountains, are considerably less, and that even M. *Cassini's* own numbers, which yet the Dr. has by some undoubted experiments shewn to be too great, fall often short. It will be sufficient to mention two or three instances. At the tower of *Massane* in *Roussillon*, the mercury stood at 25″ 5‴; and the height of that place was determined trigonometrically of

	Toises
	397
Now 25″ 5‴, answer according to *Mariotte*, to	342 0′
According to *Cassini* to	392 4′
According to Dr. *Scheuchzer*	350 0′

At the top of the mountain call'd *La Coste* in *Auvergne*, the mercury stood *Oct.* 9. 1700, at 23″ 4‴; and the height of this mountain was determined trigonometrically 851° toises.

Now 23″ 4‴ answer according to *Mariotte* to	644°	1′	diff.	206° 5′
Cassini	826	1		24 5
Dr. *Scheuchzer*	661	5		189 1

The Difference is still more considerable with regard to the high mountain, *Mont d'or* in *Auvergue*, the height whereof was determined trigonometrically to 1040 toises.

At the top of this mountain the mercury fell, according to an observation made by F. *Sebastien Truchet*, *June* 8, 1705 to 22″ 11‴; which answer, according to

Mariotte

Mariotte to	707° 5′	diff.	332° 1′
Cassini	925 1		114 5
Dr. *Scheuchzer*	727 3		312 3

Dr. *J. G. Scheuchzer* comes now to the mountains of *Switzerland*. The barometrical observations made by his father upon several of the highest will convince us, that they rise up far above all the neighbouring ones in *France*, *Spain*, *Italy*, *Germany*. And that it must be so appears farther; because from their elevated tops they dispense their waters to all the *European* kingdoms and provinces around them: Nay, he doubts not, but that they may vie in height with the most considerable mountains in any other part of the known globe.

Swisserland itself, he means its valleys and lower parts, as they are very remote from the sea, rise also in proportion above its level. It is true, the ascent thither is but gradual, in proportion to the remoteness: At *Zurich*, for instance, which lies towards the northern borders of *Swisserland*, the mean height of the barometer hath been observ'd 26″ 5‴, which gives the elevation of that town, above the level of the sea, according to *Mariotte*, 205 toises, four foot, or 1234′; according to Dr. *Scheuchzer*, 210° 4′, or 1264′; and according to *Cassini*, 221° 4′, or 1330′. This town is distant from the mouth of the *Rhine*, which is the nearest part of the ocean, at least 375 *English* miles, or 100 marine *French* leagues; and from *Genoa*, which is nearest upon the *Mediterranean* 225 *English* miles, or 62 *French* marine leagues: So that going down from *Zurich* northwards towards the sea, the descent, or fall, is but something more than 12 foot, for a marine league of *France*, if we suppose a streight line to be drawn from *Zurich* to the sea shore in *Holland*, but it is much greater going southwards towards the *Mediterranean*, where it comes at least to 20 foot for one league: Nay, if we consider that the highest mountains of *Swisserland* lie almost directly between *Zurich* and the *Mediterranean* shores, we must allow so much more in proportion, as these mountains are elevated above the horizon of *Zurich*; and how great and sudden this elevation is, will appear by the following observations.

At *Ennen sewen gen Aweren* in the ascent of the high mountain *Freyberg*, in the canton of *Glarus*, which lies south east of *Zurich*, the mercury was observ'd *Sept.* 11. 1710, at

23″ 10‴, which gives the height of that place above the leve of the sea, according to

Mariotte ——— ——— ———	569° 2′ or	3416
Dr. *Scheuchzer* ——— ———	584 4	3508
Cassini ——— ——— ———	713 3	4275

Upon *Scherf*, one of the branches of the *Freyberg*, the mercury fell *Sept.* 12, 1710, to 21″ 8‴, which gives the height of that part of the mountain according to

Mariotte ——— ———	906° 1′ or	5437
Dr. *Scheuchzer* ——— ———	931 2	5588
Cassini ——— ———	1247 4	7486

Still higher upon *Blattestock*, another part of the same mountain, the mercury fell on the same day to 21″ 6‴ which answers according to

Mariotte ——— ———	933° 2′ or	5600
Dr. *Scheuchzer* ——— ———	959 2	5756
Cassini ——— ———	1293 3	7761

Hence from *Zurich* to the *Blattenstock* near the top of the *Freyberg*, there is, in less than three days journey, a rise of 4366 feet, according to *Mariotte*, and 4492, according to Dr. *Scheuchzer*, that is, more than three times the elevation of *Zurich* above the level of the sea.

At *Guppen ob Schwanden*, in the same canton of *Glarus*, the mercury was observ'd, *August* 5, 1705, at 23″ 4‴, which give, according to

Mariotte ——— ———	644° 1′ or	3865
Dr. *Scheuchzer* ——— ———	661 5	3971

The Dr. omits giving the numbers according to M. *Cassini's* tables, having already shewn, that they are too great.

The height of this mountain is nearly the same with that of the celebrated *Puy de Domme*; where M. *Perier* observed the mercury *Sep*. 19. 1648. at 23″ 2‴.

Upon *Joch*, a high mountain in the territory of *Engelberg*, where it borders upon the canton of *Bern*, full south of *Zurich*, the mercury stood *June* 23, 1706, at 21″ 4‴; which gives the height of that mountain, according to

Mariotte

Mariotte	——	——	961°	0′ or 5766′
Dr. *Scheuchzer*	——	——	987	4 or 5926

This mountain, tho' very high, is far from being the highest in that neighbourhood; for, next to it there rises another, called the *Titlisberg*, covered with everlasting snow; which, we may, upon a moderate computation, pronounce at least 1000 foot higher than the top of the *Joch*; and consequently, one of the highest in the country.

Upon the *Avicula*, by the *Italians* called *Monte del 'Uccello*, and by some S. *Bernhard*'s mountain (from a chapel built in honour of that saint) a high mountain in *Rhœtia*, towards *Italy*, the mercury was observed *July* 30, 1707, at 22″ 11‴; which gives, according to

Mariotte	——	——	707°	5′ or 4247′
Dr. *Scheuchzer*	——	——	727	3 or 4365

This height must be understood only of that part of the mountain, which travellers pass over; the mountain itself rising considerably above it, and the *Adula*, or Διαδεελλας of *Strabo*, *Geog. lib.* III. of which the *Avicula* is only a part, being still higher. The *Rhenus posterior*, or *hinter Rhein*, and the *Moüss* (which at last loses itself in the *Tesin*, near *Bellenzone*, not much above the entry of the *Tesin* into the lake of *Locarno*) arise in this mountain.

At *Santa Maria* upon the *Luckmannier Berg*, by some called S. *Barnaby*'s mountain, which is likewise a branch of the *Adula*, the mercury stood *Aug*. 9. 1725, as upon the *Avicula*, at 22″ 11‴, which shews the height of these places to be equal.

In the *Alp San Porta*, near the source of the *Hinter Rhein* (*Rhenus posterior*) five hours and a half from *Speluga* (*Splügen*) in *Rhœtia*, the mercury was observed *July* 29, 1707 at 21″ 4‴, where it likewise stood upon the abovementioned mountain *Joch*, whither the reader is referred for the height of this *Alp*.

At *Splügen* itself, the mercury stood the same morning early at 23″ 4‴, which gives the elevation of *Splügen* according to

Mariotte	——	——	644°	1′ or 3865′
Dr. *Scheuchzer*	——	——	661	5 or 3971

So that the fall of the *Rhine* from the aforesaid *Alp* to *Splügen*, in five hours and ½ comes, according to *Mariotte*, to 1901, and

according to Dr. *Scheuchzer* to 1955 *Paris* feet perpendicula height.

At the *Capuchins*, upon the high mountain S. *Gothard*, celebrated paſſage out of *Switzerland* into *Italy*, the mercur ſtood *June* 30, 1705, at 22″, which gives the height of tha paſſage (which with regard to the higheſt tops of S. *Gothard* lies but at the foot of a high mountain, as it were) accordin to

Mariotte	— —	852°	0′ or	5112
Dr. *Scheuchzer*	— —	875	5 or	525

above the level of the ſea.

Upon the *Furca*, a high mountain between the *Urſeren Thal* (*Urſaria vallis*,) and the upper *Valleſia* and one of th branches of the S. *Gothard*, the height of the mercury in th barometer was obſerv'd, *July* 31, 1707, at 21″ 5‴, which giv the height of this mountain above the level of the ſea, accord ing to

Mariotte	— —	947°	1′ or	5683
Dr. *Scheuchzer*	— —	973	3 or	5841

Near this mountain there are others which cannot be leſs tha 800 or 900 foot higher.

Theſe mountains, namely, the *Avicula*, the *Luckmannier Berg*, the S. *Gothard*, and the *Furca*, together with the *Grimſula*, the *Criſpalt*, the *Sempronier*, or *Sempronius Mons*, th *Adula*, and a chain of others, are the *Lepontiæ Alpes* of *Pliny* lib. 3. cap. 20, and the *Summæ Alpes* of *Cæſar de bello Gallico* lib. 3. They begin in the upper *Valleſia*, traverſe the canto of *Uri*; and ſo run on eaſt-wards, a-croſs the country of the *Griſons*, towards *Tyrol*: Their greateſt height above the level o the ſea, may be fixed in round numbers to 7500 or 8000 *Paris* feet.

The *Gemmi* (*mons Gemmius*) is a very high and ſteep mountain in *Valleſia*, over which there is a paſſage but only in ſummer time, from the *Fruttinger* valley, in the canton o *Bern*, to the mineral waters at *Leuck* in *Valleſia*. The deſcent on the ſouth ſide of this mountain is ſteep and frightful, even to the ſight, beyond what can be imagined, being a narrow path, cut on the ſide of almoſt perpendicular Precipices, ſometimes with quivering wooden bridges, or planks over the

cleſt

clefts in the mountains, and here and there ſupported with low walls: Being geometrically meaſur'd it was found of 10110 feet in length, or rather height, including its ſeveral windings and turnings. At a ſmall cottage, call'd *Zur Dauben*, a reſting place for weary travellers, being the higheſt part of the mountain which is paſſable, the mercury fell *July* 1, 1709, to 21″ 3‴, which gives the height of that place, according to

Mariotte	—	—	974° 5′ or	5849′
Dr. *Scheuchzer*	—	—	1002 0 or	6012

Not far from this cottage, is a ſmall mountainous lake, called the *Dauben ſea*, or the *Pidgeon's lake*, encompaſſed on all ſides with high mountains, the tops whereof, for their ſteepneſs, it would be impoſſible to reach. At *Kandelſtag*, the firſt village in the *Fruttingen* valley, in the territory of *Bern*, going up to the *Gemmi*, the mercury roſe on the ſame day to 24″ 2‴, which gives, according to

Mariotte	—	—	520° 1′ or	3121′
Dr. *Scheuchzer*	—	—	534 1 or	3205

And at *Mullenen* at the foot of *Gemmi*, it ſtood at 25″ 7‴, which anſwer according to

Mariotte	—	—	318° 5′ or	1913′
Dr. *Scheuchzer*	—	—	327 0 or	1962

On the other ſide of the *Gemmi*, at *Leuck*, a celebrated place for its mineral waters, the mercury was obſerv'd *July* 2, and 5, 1709, at 23″ 9‴, which anſwers according to

Mariotte to	—	—	581° 4′ or	3490′
Dr. *Scheuchzer*	—	—	597 3 or	3585

So that the cottage *Zur Dauben* riſes above *Leuck* according to

Mariotte	—	—	—	2359′
Dr. *Scheuchzer*		—	—	2427

Above *Mullingen* in the *Fruttinger* valley, according to

Mariotte

Mariotte	—	—	— 393
Dr. *Scheuchzer*		—	— 40[illegible]

And the perpendicular height of the *Gemmi*, above the lev[el] of the sea, considerably exceeds 6000 *Paris* feet.

But high above all the mountains of *Switzerland* rises t[he] *Stella* (*Piz Stail*) a steep mountain in the *Schamser* a valley [in] *Rhœtia*, or the *Grisons*, the height whereof was, by some ob[-]servations in 1709, determined by Dr. *John Scheuchzer* to 958[.] *Paris* feet above the level of the sea according to his own calcu[-]lation; or 9441, according to *Mariotte*, and 12196, according [to] *Cassini*; a height, which the *Rupicapre* or *Shamoys* themselve[s] scarce venture to ascend: And it is to these only and the lik[e] heights the following verses of *Silius Italicus* ought to be applie[d].

Cuncta gelu, canâque æternùm grandine tecta,
Atque ævi glaciem cohibent; riget ardua montis
Ætherei facies, surgentique obvia Phœbo
Duratas nescit flammis mollire pruinas.
Nullum ver usquam, nullique æstatis honores,
Sola jugis habitat diris, sedesque tuetur
Perpetuas deformis hyems——————

The mountains are much more abrupt and steep, and th[e] precipices greater to the south than to the north, and westward[s] than eastwards. Several instances of this might be given in par[-]ticular mountains in *Switzerland*, as the *Gemmi*, the *Mon[s] fractus*, &c. but it is also evidently true with regard to th[e] whole. Those are the highest mountains, which seperate *Val[-]lesia*, the Canton of *Uri*, and the several leagues of the *Grisons*, from *Savoy*, *Piedmont*, and the *Tyrol*, which lie to the south, o[r] south-east. These very countries are one continued range of hig[h] mountains, as it were, quite to the *Mediterranean*; and the lik[e] structure seems to be continued farther on into that sea itsel[f]. The *Pyrenean* mountains in like manner are but a continuatio[n] of that vast chain, which begins in the *Lepontiæ Alpes*, or th[e] mountains in the upper *Vallesia*, the Canton of *Uri*, and *Rhœ[-]tia*, and from thence spreads it self chiefly west and south.

On the contrary to the east and north they break off by degree[s] into gentle plains, which evidently appears by the vast tracts o[f] ground, which, the *Rhine* for instance, and the *Danube* encom[-]pass, before they lose themselves, the one into the *German* ocean, the other into the *Black-sea*: Whereas, the *Rhosne* on the other side does quickly, and with a proportionable velocity, reach the *Mediterranean*.

The

The same observation, with regard to the abrupt steepness of mountains to the south and west, holds true in other parts of *Europe*, remarkably in *England* and *Norway*, and more or less in other countries. And so far as our Maps, and the accounts of travellers go, the same thing is observable in other parts of the world; but most evidently in the high mountains of *Peru* and *Chili* in *South-America*, which terminate very abruptly westwards into the *Pacific-Sea*; but gradually decline to the east into immense plains, water'd by some of the most considerable rivers in the known world; particularly the river of *Amazons* and the *Rio della Plata*, which rise in the said mountains.

To conclude; from what has been hitherto said, it plainly appears that the mountains of *Switzerland* are the highest in *Europe*; and they are likewise the great store-house, whence all the countries around them are supplied with water; conformable to what the learned *Loritus Glareanus* hath long since elegantly expressed in the following verses:

Præterea caput Europes hanc esse probabunt;
Æterni Alpes nivibus, juga olympica, quorum
Porgitur in cœlum caput, & sub Tartura venter;
Et quod ad Auroram, boream, solemque cadentem
Flumina perpetuo non deficientia cursu
Parturit, illa volant & in omnia membra redundant,
Ad Zephyrum & Libyen Rhodanus, Rhenana furentem
Unda citat boream, gelidus rotat Ister ad Eurum
Diras aquas, Getico novus bospes & Advena Ponto.
Ast alios sileo, quos Italia accipit amneis
Alpibus à nostris, quæque alto a vertice montis
Agmina disparibus fundunt latissima sulcis.

An account of the rise of several of the most considerable Rivers in Europe; *by the same.* Phil. Trans. N° 406. p. 587.

THE *Rhosne* (*Rhodanus*) which *Marcellinus* calls *Maximi nominis flumen*, and *Varro fluvius inter tres Europæ maximus*, rises from two *Gletchers*, as they are call'd, or *Montes glaciales*, i. e. *huge mountains of ice*, near the *Furca*, whose height has been determined in the preceeding *Trans*. and thence runs with great impetuosity through *Vallesia*, the *Wallisserland*, forming a long valley, surrounded on both sides with huge mountains, 'till it lose both its waters and its name in the *Lacus Lema-*

Lemanus, or *Lake of Geneva*, but resuming its name again ne[ar] the town of *Geneva*; from whence it flows with a more gentl[e] descent thro' some Provinces of *France* into the *Mediterranean*.

The *Tesin* (*Ticinus*) which *Claudian* in his panegyric on th[e] *Consulat* of the Emperor *Honorius* calls *Pulcher*, takes its fir[st] rise from two small lakes upon the *S. Gothard*, and some late[-] ral sources from the *Lago sopra la Cima di Pettine*, upon [a] mountain call'd *Pettine*, the *Lago della Sella*, the Lake of *Ro[-] tom* upon the *Luckmannier Berg*, the lake of *Tom*, and th[e] lake of *Bedretto*, upon a mountain of that name: It descend[s] the *Liviner valley*, (*Lavinia vallis*) and in its way to the lak[e] of *Locarno*, receives several brooks and rivulets from the ad[-] joining mountains: It unites its waters with the *Po*, near *Pa[-] via*, and loses itself jointly with that river in the *Adriati[c]* *Gulph*.

The *Rhine* (*Rhenus*) which *Cæsar de bello Gallico* call[s] *Latissimus atque altissimus*, rises in three several branches, cal[-] led *Rhenus an uterior*, *posterior*, *&* *medius*, the further, th[e] hinder, and middle *Rhine*: The hinder *Rhine* takes its ri[se] upon the high mountain *Avicula*, *Colmen del' Occello*, part o[f] the *Adula*, in the alp *San porta*, from a *Gletcher* or ice moun[-] tain, which extends in length full two hours: The middl[e] *Rhine* rises upon the *Luckmannier Berg*, which is likewi[se] part of the *Adula*, in the upper part of a valley, call'd *Sa[nta]* *Maria*, over-against one of the sources of the *Tesin*: Th[e] furthermost *Rhine* rises upon that branch of the *Crispalt*, cal[-] led *Cima del Badut* (*Baduz*) and soon receives several latera[l] branches from the alps *Mugels* and *Cornera*, near the monaster[y] of *Discentis*; the further and middle *Rhine* join together, and the united stream falls into the hinder *Rhine*, near *Reichenau*. Below *Rheineck*, the *Rhine* falls into the *lacus Bodamicus* o[r] *Boden* sea, and comes out of it near *Stein*; from whence wash[-] ing for some time the borders of *Swisserland*, it then traverse[s a] great part of *Germany*, in a very irregular course, 'till at la[st] in *Holland*, it loses itself in the great ocean.

The *Reuss* (*Rusa*) rises from a small lake called *Lago d[i]* *Luzendro*, upon the *S. Gothard*; but soon receives a considerabl[e] re-inforcement from the *Furca*; and near *Urselen* anothe[r] from a mountainous lake in *Oberalp*. Near *Fluelen*, not fa[r] from *Uri*, it enters the IV. *Waldstetin sea (lacus quatuor civi- tatum sylvestrium)* but resumes its course and name at *Lucern*; and at last falls into the *Aar*, below *Windish* (*Vendonissa*.)

The

The *Aar* (*Arola*, *Arula*) rises upon the high mountain *Grimsula* in the upper *Vallesia*. About three hours below that it falls into the lake of *Brientz*; and out of that, not far from the monastery *Interlachen*, into the lake of *Thun*, which it leaves near the town of *Thun*; and from thence running by *Bern*, *Solothurn*, and so down, falls at last, after several windings and turnings into the *Rhine* near *Coblentz* (*Confluentia*) probably so called from the uniting of these considerable rivers.

The ascent of the mountains of *Switzerland*, being so very sudden and quick, that as has been shewn in the preceeding *Transaction*, the elevation of the mountains in the canton of *Glarus*, above the horizon of *Zurich*, tho' not quite three days distance, is more than three times as great, as the elevation of *Zurich* itself above the level of the ocean, from which it is upwards of 375 *English* miles distant in a streight line; and so in proportion of others; and the rivers, which rise in these mountains, rushing down, in consequence of so quick a descent, with great force and impetuosity, it was to be feared they would often overflow their banks, and cause frequent inundations in the flat plains in *Swisserland*, if this force and impetuosity were not in great measure broken, and their waters disposed to a more gentle descent: And this is actually done by those great receptacles of water, namely the lakes; which, besides, are of vast use to the inhabitants around them, supplying them with plenty of fish for their sustenance; and enriching them by the facility, with which commerce may be carried on over them.

Thus the *Rhine* falls into the *lacus Bodamicus* (*Boden sea*) the *Rhosne* into the *lacus Lemanus* (or *Lake of Geneva*) the *Meusa* and *Thesin* into the *lake of Locarno*; the *Reus* into the *Lake of Lucern*; the *Adda* and *Maira* into the *Lake of Como*, the *Lint* or *Limat* into the *Lake of Zurich*; the *Aar* into the lakes of *Brientz* and *Thun*. And it seems, that the more considerable the rivers are, and the more impetuous their course, so much the greater must the receptacles be, in which they are to lose their force and rapidity. The *Lake of Geneva* and the *Boden sea* (the two largest in *Swisserland*) do evidently evince this; and the others above-mentioned gradually decrease in largeness, in proportion as the rivers, which fall into them, become gradually less rapid.

Of the smallness of the Alpine Plants; *by the same.* Phil Trans. N° 406. p. 593.

THE *Alpine* plants become still smaller in proportion, a the mountains upon which they grow, rise higher: Whe ther this be owing to the sharpness and purity of the *Alpine* ai or the decreasing pressure of the atmosphere, which is far les upon mountains than in valleys and lower countries; or to th want of a sufficient quantity of subterraneous heat, to push th nourishment into the roots and vessels of the plants, or rather t a joint concurrence of these and other causes, would require more leisurely consideration.

The thing itself is an indisputable matter of fact, and i also extends to trees and shrubs, which become smaller, as the grow on higher places: Nay, what is still more remarkable, n trees will grow beyond a certain height, which is the reaso why the tops of mountains appear so bare and naked, if view' at a distance, tho' a curious traveller shall not fail meetin upon their rich pastures with an agreeable variety of beautifu plants. The height, where trees cease to grow, hath bee found, by barometrical observations, to be nearly the same i divers parts of *Swisserland:* In other respects, the smallness o the *Alpine* plants is abundantly compensated by the richness o their virtues, which are purposely centred there, as it were into so narrow a compass.

Experiments occasioned by S. Rizzetti's Optics; *by Dr.* Desa guliers. Phil. Trans. N° 406. p. 596.

S. *Rizzetti*, an *Italian* Gentleman, published a book, *Ann* 1727, on the affections of light, in opposition to Sir *Isaa Newton*, and which he dedicated to Cardinal *Polignac*. In i he calls several of Sir *Isaac Newton*'s experiments into question because they did not succeed in the way he tried them, denie the consequences of others he allows of, and advances new hy potheses contrary to experience.

The president of the *Royal Society* being acquainted wit this, desired Dr. *Desaguliers* to make some experiments on thi occasion: How those were made and how they succeeded, wit the inferences from them, you have in the following account.

N. B. Some of these experiments are Sir *Isaac Newton*'s, but made after a different manner; and some, as the Dr. in forms us, entirely his own.

Expe

Experiment I. The Dr. prepared a box (represented Fig. 1. Plate XI.) of about three foot high, and one foot wide within (whose shape was a truncated pyramid) in the following manner: He painted the inside of it black, and in the back part, a foot above the base, he made a square hole three inches wide (whose section is *r r*) to receive a piece R, shutting close with a rabbet or shoulder, whose surface coming thro' the hole was entirely covered with the painted paper, on which the experiment was to be made: Over against *r r*, in the fore-part of the box, was a door to open with a tube in it, four inches wide and five inches long, whose section is *e f g h*, that two candles set on the places *i k*, to enlighten the paper at *r r*, might throw no direct light out of the box, whose section is represented at *a b c d*. Then having made the room perfectly dark, he fixed the box upon a table, that it might remain in one place; at the distance of eight foot from *r r* he fixed the *lens* L L, of four foot *Focus*, in a frame upon another table, with its axis going thro' the middle of *r r*: At the distance of about eight foot beyond the *lens*, he set up the skreen, or square of white paper S. Having put into the hole *r r* a stiff paper, painted with vermilion, and wrapped four times and ½ with black silk (as represented by R) that paper enlightened by the candles at i, k, the image of the red paper was projected upon the skreen at *p*; and when the most distinct place was found, the skreen was fixed: Then a paper, painted with ultramarine, being fixed in the hole *r r*; the image of it was so distinct at *p*, that the images of the black silks could not be seen; but holding a piece of paper close to the skreen, and bringing it forwards, at about ¾ of an inch from the skreen, the representation of the silks began to appear on the blue image; but it was most distinct at 1 inch and ¾, or at Z Z: So that there was 1 inch and ¾ between the distinct base of the red, and that of the blue paper.

But what has laid several people into an error in making this nice experiment is the depth of the *Focus* of the rays in both cases; for tho' the red image was most distinct at *p*; yet the representation of the black silks might just be perceived by a good eye, when the skreen was moved backwards or forwards ¾ of an inch. The blue image, which was stronger, had its silks visible an inch on either side of Z Z: So that in a paper half-red and half blue, painted with these colours one might have seen the silks (tho' faintly) upon the two images at once,

and have been thereby deceived: But ¾ of an inch beyond the place common to both, the red alone would have appeared distinct, and an inch short of the said place, the blue image most distinct, and that alone so; that is, an inch and ¾ nearer the glass. Instead of vermillion the red paper may be painted with *carmine* or *lac*; but it will not do so well, as was then tried; nor does *Prussian* blue do so well as ultramarine. The best way is to heighten the vermillion with a little *carmine*; and the ultramarine (which has too much white) with indigo; and then there will be a space between the two distinct bases, where both the images will be in distinct.

N. B. The Dr. made the experiments with such colours in the year 1722; but now he used no mixtures, that any body else might repeat the experiment.

Fig. 2. represents the box with one side out, whose place is $\delta d b \beta$; eg is the hole for the tube in the door of the foreside $\varkappa \delta c d$; rr the hole in the back to receive the piece R with its painted paper.

Fig. 3. represents the box open before, with the candles and paper in it, the same parts being marked with the same letters, as in the other figures.

N. B. The Dr. made the experiment in this manner; because S. *Rizzetti* attributed the different *foci* of the colours to different inclinations, which could not be alledged here; the red and blue being, as the latter had desired, successively fixed in the very same place. And he says p. 64. *Addidi permanentes colores a lumine directo diversâ inclinatione illustratos, constante inclinatione in lentem incidere.* Nay more than this was performed in the experiment: For, as the candles were fixed, the light fell upon the painted paper always with the same incidence.

Exp. 2. Instead of the red or blue paper at r r (Fig. 1, 2.) the Dr. fixed upon the piece R a paper half red, and half blue, as R B (Fig. 4.) then over the hole in the fore-part of the box, represented by *eg* (Fig. 2.) he fixed a square plate $\varkappa \delta c d$ (Fig. 4.) with an oblong hole in it four inches long in its horizontal position, and an inch deep, thro' which one might see the parti-coloured paper, as if it were only of the bigness, and figure of this aperture, and strongly enlightened by the candles, hid in the box, the rest of the room being very dark.

N. B. The Dr. made this preparation; because S. *Rizzetti* objects to Sir *Isaac Newton*'s first experiment of the first book, that

that the black cloth beyond the parti-coloured paper was not colourless; and therefore the experiment was not decisive, as particularly relating to the paper.

R B (Fig. 5.) represents the paper, contracted in length and breadth by the aperture of the plate, which paper being look'd at, at the distance of five foot, by the prism 1, appear'd, as drawn at r b: The prism being removed to 2, at the distance of 10 foot, represented the paper, as at *r b*; and when it was at 3 (at the distance of 15 foot) the paper appeared as ρ β. In these three cases, the blue b, *b* and β appeared lower than the red r, *r*, ρ, the refracting angle of the prism being downwards. When the refracting angle was held upwards, as at 5, then the blue B was raised higher than the red R: But if due care be not taken, in turning the prism, a reflection may be mistaken for a refraction, as at 4; and then, it is true, the red and blue will be equally raised, as at T. This must have been S. *Rizzetti*'s mistake, when (in p. 38.) he says, that one colour was raised higher than the other by two lines, at 10 foot distance, but not at all at five foot: For, several of the persons, present at the Dr's experiments, made the same mistake at first, before they could perform the experiment in the manner abovementioned; which they at last did; and found the colours separated most at the greatest, and least at the least distance.

This mistaking a reflection for a refraction has been the occasion of several more errors and difficulties to be met with in S. *Rizzetti*'s book.

Exp. 3. A candle K (Fig. 6.) reflected from the surface A B of the prism A B C, appeared very faintly to the eye at E, as a weak image at k; because the rays incident at I pass most of them thro' the prism, and go on to R, separating from one another, according to their different degrees of refrangibility; whilst a few of them are reflected to the eye in the direction IE.

But if the prism be in the position A C B (Fig. 7.) most of the rays of the candle K, incident at I, on the plane A B (after having passed perpendicularly thro' the plane B C) are reflected; and passing perpendicularly thro' A C, go into the eye at E, which sees a very strong image of the candle at k, whilst very few rays go down to R to produce colours.

Corol. This shews, that the rays of light pass with more facility thro' glass (a dense) than thro' air (a rare) medium; contrary to *Rizzetti*'s assertion.

Exp.

Exp. 4. To make this more evident, and compare togeth the facilities with which light passes thro' the two mediu the Dr. took a cube of glass three inches the side A *a b* B *d* D (Fig. 8.) whose section is A B C D, and looking upon it from to see by reflection the candle K, he saw two images of it; o at *k* very faint, and reflected from the upper surface A B, a the other at *x* very strong, and reflected from the lower surfa C D. Now it is evident, that the vividness or brightness of t image *x*, is to that of the image *k*; as the facility with whi the rays in these circumstances pass thro' glass, or thro' ai And these are easily compared; because both the images a seen at once.

Exp. 5. The line P I (Fig. 9.) being perpendicular to the r flecting plane A B of the triangle A C B, the Dr. brought t candle K by degrees so near P, as to diminish very much t angle of incidence K I P; which made the image, or appea ance of the candle at *k*, become fainter by degrees and at last faint, as in Fig. 6.

Exp. 6. The Dr. having made the experiment as represented Fig. 7. he pressed another prism D F G (Fig. 11.) close to t prism A B C; and when he squeez'd them together but gentl some of the rays from the candle K passed thro' the low prism, and falling upon a paper at R, formed a reddish spo But when he squeez'd them very hard, the spot became muc wider, white in the middle, and only tinged with red about t edges: At the same time the eye saw a black spot in the imag of the candle at *k*; and a by-stander looking obliquely at t place I (where the glasses touch'd) saw thro' the prisms a lit hole, as it were, as big as the spot *k*: But if the prisms pressed together but gently, then all the other phenomena disa pear, excepting the first little spot at R, as in Fig. 11.

When the candle is seen by reflection from the lower surfac of a prism (as in Fig. 7, 9, 10.) the rays pass quite thro' th surface, and are turned up again by its attraction in curve line so as to re-enter the prism; and then (going out again thro' t surface A C) go up to the eye at E. In this case the most r frangible rays, being the most easily inflected, form the lea curves, whose vertex's are nearer the glass than those of t larger curves, formed by the least refrangible rays. This proved by *Exp.* 6. where the under prism only attracts dow from the reflection of the upper prism; the red, forming ray as in Fig. 11. where the plate of air between the prisms is some small thickness. But when the prisms, whose surfac ar

are ſomewhat convex, are preſſed hard together, the lower priſm is near enough to attract rays of a great degree of refrangibility; and therefore, the ſpot then becomes white in the middle, and only red about the edges, which are produced by ſuch parts of the lower priſm, as are not ſo near the upper priſm.

There are two circumſtances in *Exp. 6.* which diſprove *Rizzetti*'s aſſertion p. 125, viz. *that there is a ſenſible reflexion, even where glaſſes touch*: For, when the priſms touch at I (Fig. 10.) the black ſpot appearing in the image of the candle *k*, ſhews, that there is at I a deficiency of thoſe rays, which, coming from the middle of the candle, uſed to be reflected up to the eye at E; and therefore, that A B, the reflecting ſurface of the upper priſm, ceaſes to reflect in a little ſpace round about I, where the upper ſurface D F of the under priſm touches it; the rays, which before were reflected, now going down to make the ſpot at R: The other circumſtance is this; that whereas a paper *k* is inviſible to an eye at E, by the interpoſition of the priſm D F G; when another priſm A C B is laid over it, and preſſed hard, there appears to be a hole of about $\frac{1}{5}$ of an inch (more or leſs in diameter, as the priſmatical ſurfaces are more or leſs flat) thro' which the paper at *k* becomes viſible; this being the place of contact, where the reflection downwards (of the ſurface D F) ceaſes.

This happens, becauſe thoſe rays, which, coming from the candle K, were bent in curves under the ſurface A B of the upper priſm, about ſeveral points near I, are by the nearneſs of the ſurface D F of the lower priſm brought down to R, inſtead of being turned up again to the eye at E; whilſt thoſe rays, which (coming from the paper at *k* thro' the ſurface G F of the lower priſm, and paſſing thro' its upper ſurface F D) were bent in curves about ſeveral points near I, are prevented from turning down again to R, and are brought up to the eye at E; which conſequently muſt ſee a round part of the paper at *k*, juſt as big as the place of contact, which appears like a hole; or as if the two priſms being changed to a parallelopiped, were cover'd with a dark paper, that had only a ſmall hole in it.

But to make this more evident, eſpecially to ſuch as are not acquainted with Sir *Iſaac Newton*'s optics, the Dr. explains the manner of the bending of rays, where they are refracted or reflected.

Of

Of the bending of rays in the refraction.

Schol. 1. Let DD (Fig. 12.) represent a dense medium, glass, whose surface is GG, and AA a rare medium, as a Now let us suppose a power to extend all over the surface G acting from AA towards DD in lines perpendicular to the su face GG, very strong in contact, but insensible at a very sm distance from the said surface, which we will call the attracti of the surface GG, without considering whether it be any re virtue in the said surface, or the action of a medium impelli towards it: Let lines 11, 22, 33, such as express the lines which the attraction exerts itself, and the line MM (exceedi near to GG) the limits of the attraction, beyond which it ca not affect a ray of light. Let the ray of light R*a* movi from a rare medium into a dense one in the direction Rr, co towards the surface GG in such an angle, as that it may refracted. When the ray comes to *a*, by the attraction at *a* will be acted upon in the line *a*b; and (by the known laws mechanics) be turned out of the way into the direction *a*a, i stead of *a*r: When it is got to *b*, being acted upon in the d rection *b*4, its new direction will become *b*b: At *c*, by t power acting in the line *c* 5, it will change its direction to *c* and lastly, at d it will go into the glass in the line *d*d, con nuing in that streight line, whilst it moves in that medium.

Now if the lines 11, 22, 33, *n*, *c*, *b*, *a*, be infinitely near (they must be supposed to be) the ray, instead of being brok into the several streight lines *ab*, *bc* and *cd*, will be bent in the curve *abcd*, and the emergent ray *d*d will form the sam angle with the incident ray Rr, as if the refraction had be made at once at the point *n*, which point may be considered in the surface GG, because MM has been supposed extreme near that surface: Then also may refractions be considered grofs, and rays traced in all optical propositions, as if the were no such curve, as what the Dr. has been describing.

Again, let D (Fig. 13.) represent the dense medium, glass; and A the rare medium, or air; R*a* a ray of lig coming out of the dense medium into the rare, in the directi R*r*, in which it may be refracted (as for instance, in an ang of 30 degrees with the perpendicular P*a*) let MM be the li which limits the attraction of the surface GG; which attra tion is exerted in lines, tending perpendicularly from MM GG: As soon as the ray of light hath emerged at *a*, it

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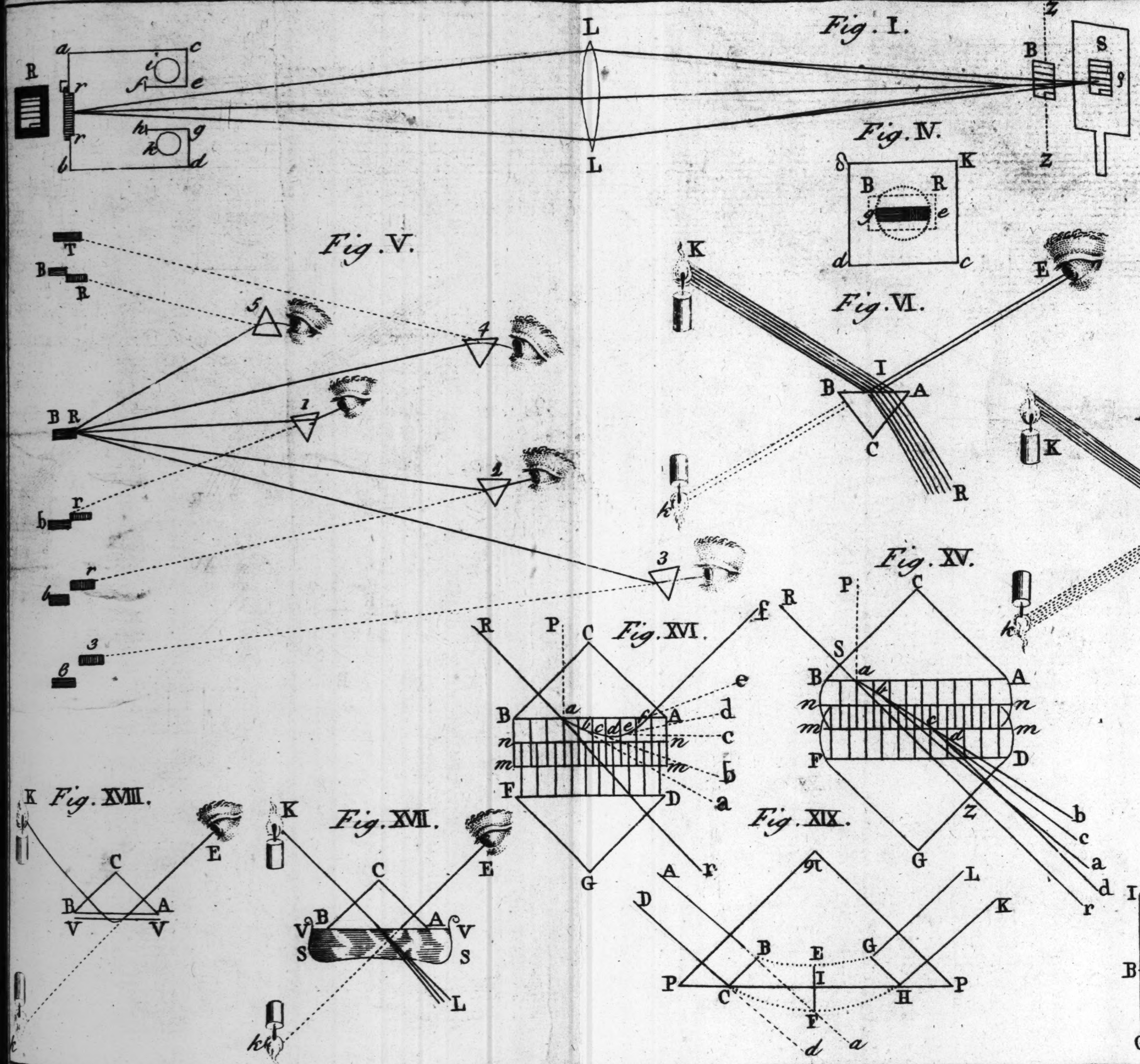
PLATE. XI.



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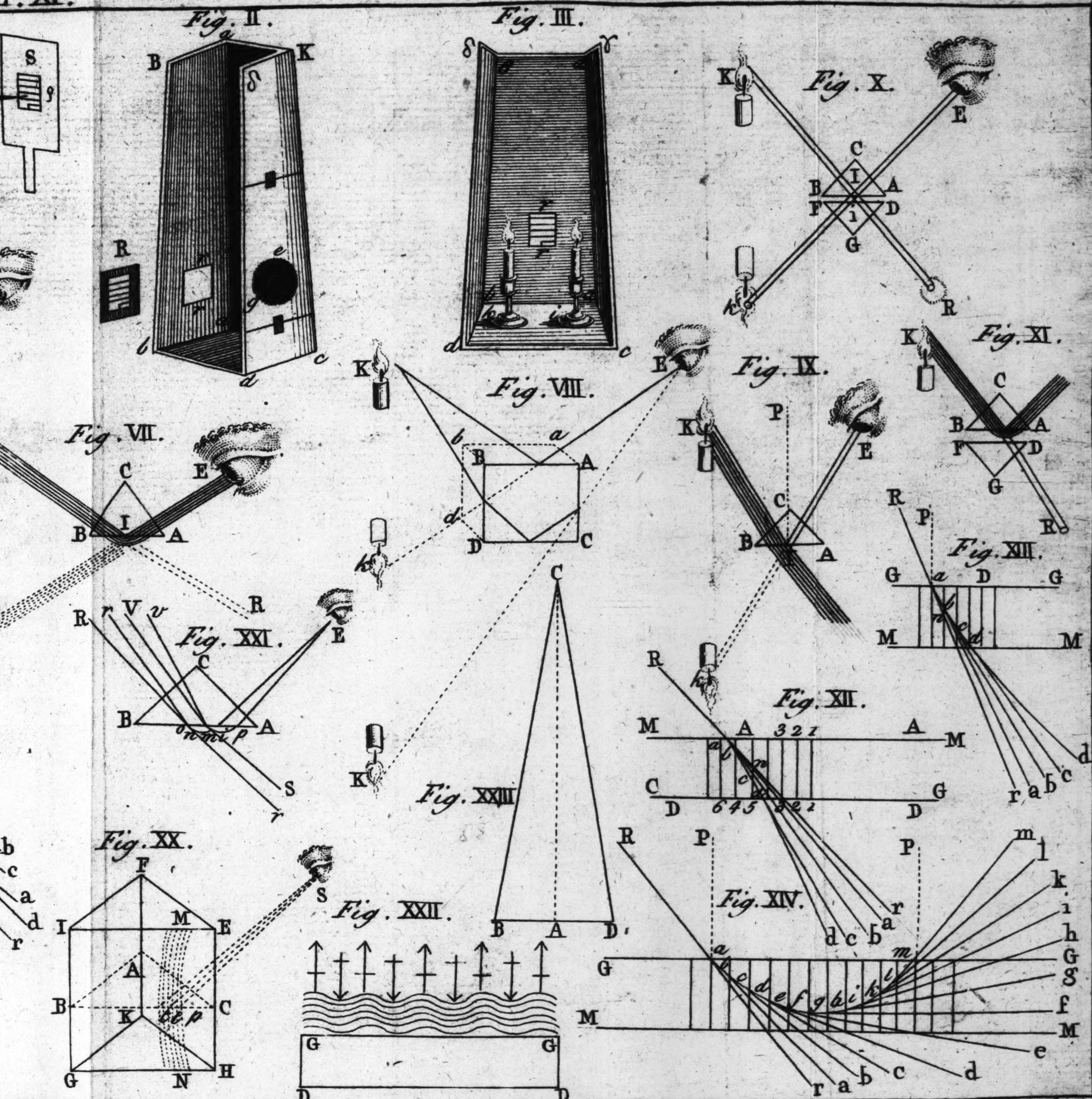

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attracted in the direction *a* P; and therefore diverted from the line *a* r, into the new direction *a* a; at *b*, it is turned into the line *b* b; at *c*, into the line *c* c; and at *d*, into the line *d* d: So that the emergent ray will be *d* d; as if the refraction had been performed in the point *n*, and that point in the surface G G, without any curve at *a b c d*; and all the rest as we have considered it before, only with this difference, *viz.* that the ray is bent just as it comes out (or rather when it is come out) of the dense medium: Whereas before, we considered its bending, before it came into it.

Of the bending of rays in reflection.

Schol. 2. But if the ray R *a* (Fig. 14.) coming out of glass into air, should come in such a direction, as to be entirely reflected, as it will do, when the angles R *a* P is 45 degrees: In that case the reflection will not be made at the surface G G, nor above it in the glass; but under the said surface, in air, or even *in vacuo*, or any medium less dense, or rather less refractive than glass.

M M represents the limits of the attraction of the glass, exerted in a direction from M M to G G perpendicularly, as was said before.

The ray R *a*, moving in the direction R r, at its emersion at *a*, is, for the reasons before given, turned into the direction *a* a; then at *b*, into the direction *b* b; at *c*, into the direction *c* c; at *d*, into the direction *d* d; at *e*, into the direction *e* e; and at *f*, into the direction *f* f, parallel to G G; then at *g*, the ray is again turned towards the glass, by whose attraction changing successively into all the directions, *g* g, *i* i, *k* k, and *l* l; at last it re-enters the glass in the direction *m* m, forming the same angle with the perpendicular *m* p, that R *a* formed with *a* P. Now as the lines perpendicular to G G, drawn from M M, are infinitely near, the line *a b c d e f g h i k l m* must be a curve; and as M M, and G G are extremely near, the vertex of the curve (whose tangent is *f* f, parallel to G G) will be so near the point I, as to be considered, as co-inciding with it, when we compare the angle of incidence with that of reflection; then also will the space between the parallels p *m* and P *a* be so far diminished, that those two lines may be looked upon as co-inciding, the angles m *m* p, and R *a* P being equal, whether the three points, *m*, I, *a*, co-incide or not.

For these reasons, for common use, one may consider the reflection from the under surface of the glass as made at once in

that ſurface at the point I: But when we examine thing ſtrictly, experiments, as well as the above reaſoning, will ſhew, that there is ſuch a curve, as we have mentioned (*vide Exper.* 6. Fig. 10. 11.) and we ſhall mention others.

N. B. If any point of the curve *a b c &c.* between *a* and *f* fall below (or beyond the line MM) the ray will then go on in a ſtreight line, tangent to the curve in that point, where it leaves the line MM.

Now let us ſuppoſe M e d c b a r M (Fig. 14.) to be glaſs, or any other denſe medium, and m p P R to be air, or any other rare medium, and R *a* a ray of light, moving in the rare medium towards the denſe medium, in the direction R*a* towards r: if inſtead of an attraction at the ſurface of the glaſs MM, there be ſuppos'd a repellent force, whoſe limits are GG, then will the ray by the repulſion of the ſurface MM be bent into the curve *abcdefghiklm*, in the ſame manner as we ſhew'd it would be under the ſurface GG, when G p P G was conſidered as a denſe medium. Hence it follows, that a ray, moving in the air, is reflected from a ſpecular ſurface of glaſs, or any other mirror, opaque, or diaphanous, without touching the ſaid ſurface.

N. B. That the ſame power may, under different circumſtances, attract to, and repel from, the ſame ſurface, ſhall be made out in the remaining part of this Tranſaction: But now taking ſuch a power for granted, we will proceed in conſidering the flexure of rays of light.

Let us ſuppoſe a priſm A B C (Fig. 15.) to have the attracting power of its inferior ſurface extend as far as the line *mm*; if another priſm G D F (the attracting force of whoſe upper ſurface extends as far as *nn*) be brought very near to the firſt priſm; where the attracting powers of the priſms interfere, they will deſtroy each other, becauſe they act in contrary directions; and thereby the limits of attraction of each of the ſurfaces will be contracted; the power of AB extending no further than *n n*, and that of D F no farther than *mm*, whilſt the ſpace *nnmm* loſes all the force that it had (and would have upon the removal of either priſm) to turn a ray of light, moving obliquely, out of its direction.

Now in this ſituation of the priſms, a ray of light, entering the ſurface C B at right angles, will likewiſe go thro' the ſecond priſm at right angles (not exactly in the ſame line, but) in a line, parallel to the direction of the incident ray. For inſtance, let the ray R*a* (not refracted at, becauſe

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perpendicular to the ſurface CB) emerge out of the firſt priſm at *a*, in the direction *a r*; its changed direction at *a* will become *a* a; and at *b*, *b* b; or rather the ray will be inflected in the curve *ab*; and at *b* getting out of the power of the attraction of the ſurface A B, it will, (for the reaſons before given) move in a ſtreight line from *b* to *c*, where it will be bent again the contrary way in the curve *c d* of the ſame kind as *ab*; and laſtly emerge in the direction *d* d, parallel to the firſt direction R r. Hence it follows, that when the priſms are brought ſo near as to touch, their mutual attractions deſtroying each other, the rays of light will not be bent, but paſs thro' the two priſms (which in this caſe perform the office of a parallelopiped) in the ſame direction with which they came into the firſt priſm; and conſequently, produce no colours: Contrary to what is affirmed by *Rizzetti* p. 78, 79, *&c.* and when the rays R*a* fall obliquely upon the ſurface C B; the effect of their refraction at their immerſion at S to produce colours, is taken off by the refraction, which they ſuffer at their emerſion at *z*.

Exp. 7. The Dr. took the cube (repreſented Fig. 8.) and looking obliquely thro' it at the hole of the window of his darkened chamber (the ſun ſhining or not ſhining) the hole appeared entirely colourleſs, as did likewiſe a candle; both appearing fringed with colours, when ſeen thro' the priſm. Then holding two priſms together (as in Fig. 10.) if the hole of the darkened chamber be at k, it appears white to the eye at E; but if the angles of the priſms at B F be ſomewhat ſeparated, whilſt the points A D touch, the hole will appear colour'd: When the ſurfaces are ſeparated at A D, and touch at B F, the colours appear in one inverted order; but if the ſurfaces A D and B F are parallel, whether they touch or not, the hole will appear white.

N. B. In this caſe the priſms muſt be ſimilar, that the ſurface F G may be parallel to A C; otherwiſe A B and D F muſt be inclined to each other in ſuch a manner, as to render A C and F G parallel. It is true, if one of the priſms be very far remov'd from the other, the heterogeneous light, which entered in at F G, may be ſo far ſpread by the ſeparation of the differently refrangible rays, that the priſm A B C will not take it all in; then the eye behind the ſecond priſm may ſee colours, as the Dr. ſuppoſes *Rizzetti* did. *Vide* p. 79. of his book.

If the ray of light R*abcdd* (Fig. 15.) changing its direction in the manner abovementioned form an angle of about 45 degrees with the perpendicular P*a*; upon the removal of the lower prism, the ray will be turn'd up again, as in Fig. 14. but if the angle P*a*R be greater, the ray will still be turned up again in a curve, as *abcdef* Fig. 6. notwithstanding the lower prism is at DFG: But if that prism be brought up closer to the surface AB, the curves will be destroy'd where the prisms touch; and all the rays in the place of contact brought down thro' the lower prism.

The most refrangible rays consist of smaller particles than the least refrangible ones do; and therefore, must have least *momentum*, the velocity of all the rays being the same; and consequently, are more easily turned out of the way by attraction or repulsion; which makes the curves, formed by the purple and violet rays under the surface AB, to be less and nearer the said surface, than the curves formed by the red and orange rays.

Suppose a violet ray moving in the direction Rr Fig. 16. to be bent under the surface AB, in such a manner that at the vertex of the curve, or where its tangent *cc* is parallel to AB, there still remains a small space between the curve and the line *nn*, where the limits of attraction (contracted by the proximity of the undermost prism DFG) end, that ray will be turned up again in the curve *def*, and so reflected in the line *ff*; the directions having been successively changed, as in Fig. 14. But a red ray with the same inclination would pass on into the lower prism, as was explain'd in Fig. 15. Because the *momentum* of the red ray being greater than that of the violet, the same degree of attraction could not give it the same flexure.

This is confirmed by experiment: For, when the lower prism is not press'd hard against the upper, as in Fig. 11. the rays brought down to R form a spot of a colour, chiefly made up of red and orange rays; but when the prisms are press'd closer, the spot grows larger, and perfectly white in the middle, because all sorts of rays are brought down to the spot; but it is inclos'd round with a reddish border, occasioned by the parts of the prism which are very near, but not in contact, or at least not near enough to bring down the green, blue, purple and violet rays.

This

This shews that the reflection is not made from the interior solid parts of the glass, nor from the parts in the surface as *Rizzetti* affirms. But this is made more evident by

Exp. 8. A candle being in the position K (Fig. 17.) the eye at E and the prism at A B C; a strong image of the candle was seen at k, as in Fig. 7. But lifting up a vessel of water VSSV, till the surface of the water V V touch'd A B, the lower surface of the prism; the image of the candle became almost insensible, as the eye lost all those rays, which now were attracted into the water. And for a further proof, that the reflection is made under the surface, and not in it; when the prism was taken out of the water, being wet at its lower surface, or having a *stratum* or *lamina* of water (whose surface was V V, Fig. 18.) under A B; the image of the candle did again become vivid; the rays being turn'd up again under V V. The image, it is true, in this case, tho' strong, did not appear well defin'd, by reason of the unevenness of the watery surface V V Fig. 18.

The Dr. is very well aware, that *Rizzetti* may answer here that what he has said above does, in some measure, favour his notions; and that the rays, which (in Fig 7. having pass'd thro' A B, the lower surface of the prism) are turn'd up again to the eye at E, do not suffer a reflection, but a new immersion: For, says *Rizzetti* in p. 125.— 'The *English* Gentleman (*meaning Sir Isaac Newton*) subjoins in the 'second place, that if light, in passing from glass into air, 'should fall more obliquely than in an angle of 40 degrees, it 'is entirely reflected'. To which I answer that from what I have laid down in *Prop.* 4. *cap.* 1. *Opt.* 1. it follows, that 'this is not a true reflection of the light, but rather a new 'immersion; and therefore I deny that it follows from that 'phenomenon, that light is reflected from the solid parts of 'bodies, at some distance'. And a little lower, having quoted what Sir *Isaac Newton* says, concerning the blue light, which coming from one prism obliquely upon the farther surface of another, is entirely reflected, at the same inclination that the red light is entirely transmitted — he says; 'Let it again suffice to answer, that in this case likewise 'what the author calls a reflection, is a new immersion of 'the light'.

But this is only cavilling about words: For, if the ray of light, which moving in a dense medium, falls obliquely on the surface, common to that and a rarer medium, be turned back

back again in the denſe medium; ſo as to make the angle in which it returns from the ſaid ſurface, equal to that in which it came to it; this return of the ray may properly be call'd a reflection, whether the ray be turn'd back at the point of incidence in the ſurface, or be carried about the point of incidence in a ſmall curve, the conſideration of which may be omitted, in tracing the way of a ray of light in its paſſage for making of optical machines. Whoever reads the 8. prop. of the ſecond part, book 2. of Sir *Iſaac Newton*'s optics may very eaſily find that he was not ignorant of the turning back of the ray under the ſurface of the glaſs, before it return'd into it: And tho' the reflection in that caſe be not made by impinging on the ſolid parts of the glaſs; yet it is owing to them, that the light (acted upon at a diſtance) is turned up again, as has been ſhewn by ſeveral of the above-mentioned experiments.

Now let us ſee how *Rizzetti*'s account of the new immerſion agrees with phenomena.

Let all above the line P P (Fig. 19.) be a denſe medium, as glaſs; and all below it a rare medium, as air; A B C D is a beam of light of inſenſible thickneſs, but of ſome breadth, whoſe rays cohere to one another, and whoſe ſection or firſt line is B C. If the medium, in which B C is, did not change, B C would move parallel to itſelf in the lines B *a* and C *d*; but as the extremity C of the line B C comes out into a rare medium, which being of leſs reſiſtance to light (for, ſo he ſuppoſes) the point C, moving with more facility than the point B, deſcribes the curve C F H, whilſt B moving in the denſe medium with more difficulty, deſcribes the leſſer curve B E G, then the point C being got to H is re-immerſed, and the line B C being got to H G goes on in the direction HKGL parallel to itſelf, drawing the beam after it in a rectilinear direction, after part of it has been bent within the glaſs, and part of it without.

Now, if this be true, and P P π be a priſm, Dr. *Deſaguliers* would fain know, what becomes of the line at E F, which unites the rays of the beam about the point of incidence I, when water is brought to touch the ſurface P P, as at A B, Fig. 17? If it be ſaid, that water making a great reſiſtance, tho' not ſo great as glaſs, the curve B E G deviates ſo little from the line B *a*, that the point E comes below I, and the beam is entirely refracted; the Dr. aſks, from whence comes the faint image at k? If it be anſwered, that ſome part E

of the line E F (Fig. 19.) is turn'd up to the eye at E (Fig. 17.) what becomes of the lateral cohesion of light, on which *Rizzetti* founds his chief proposition, and from which he draws his consequences.

It would be tedious, as well as useless, to be particular in shewing all *Rizzetti*'s mistakes: The Dr. therefore only mentions one more experiment from Sir *Isaac Newton*, which he repeated on account of what is said in *Rizzetti*'s preface, p. 16. *viz.* ' that if (according to Sir *Isaac Newton*) ' rays were differently reflexible, colours must be produced ' by reflection from a plane surface; but this, says our ' author, is contrary to experience'. Now this his assertion is disprov'd by

Exp. 9. Which as it was made exactly in Sir *Isaac Newton*'s manner and with the same success, the Dr. refers to his own account of it in book 1. part 2. exper. 16.

If this account need any farther explanation, let us suppose CAB the section of the prism in Sir *Isaac Newton*'s, Fig. 13. transferr'd to Fig. 21. at ACB. If R*o* be a red ray, inclin'd to a perpendicular to AB, in an angle of more than 41 or 42 degrees, it will at its emersion under the surface A B be turn'd into the curve *o n m i*; and so go up again to the eye at E; but another red ray coming in the direction *r n*, forming an angle with the perpendicular sufficiently less, will after its emersion at *n*, be only bent so much, as to be turn'd out of the way, and refracted to *q*, in an angle of refraction agreeable to the refrangibility of red light: But V*m* a violet ray with the same inclination as the last red one *r n* shall not be refracted, but turn'd up in the curve *m i* P; and so go to the eye at E: Another violet ray v*m*, forming an angle something less with the perpendicular, will pass thro' the glass, and be refracted in the line *m*S. Upon this account all that part of the base of the prism (of which AB is the section) between A and *p* will be dark, or faint; all that part between *p* and *n* be tinged with a bluish colour, and all between *o* and B be of a bright white.

The bending of rays of light just as they come to be reflected, or refracted, may be easily understood by such as are well acquainted with those properties of light, which Sir *Isaac Newton* calls their *fits of easy reflection*, and *fits of easy transmission*; without any hypothesis, by consequences fairly drawn from experiments, and observations. But as S. *Rizzetti* does not seem to have the least notion of those

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properties of light, and the nice obſervations, upon which they are founded; and as ſeveral other perſons have not time enough to read thoſe parts of the optics with ſufficient application; to ſhew how the ſame power of the ſurface of a denſe medium may both attract, and repel under different circumſtances. — The Dr. contents himſelf here with giving the hypotheſis, which Sir *Iſaac* does, before he comes to that part of his book, where he demonſtrates the above-mentioned fits.

If GG (Fig. 22.) be the ſurface of a denſe medium GDDG, on which a tremor is caus'd by the warmth communicated to it by the rays of light; ſo as to give a wave-like motion to the ſurface GG; as that vibratory motion is perform'd, the medium alternately puſhes from the ſurface, and returns towards it (as repreſented by the poſition of the darts in the figure) and puſhes back the light: So as to reflect it when the vibration is contrary to its direction; but brings it down to be refracted, when the vibration conſpires with the ſame motion, *Vide* Sir *Iſaac Newton*'s optics book 2. part 3. prop. 12.

The Method of making Tin-plates, *extracted from the Memoirs of the Royal Academy of Sciences for the Year* 1725; *by Dr.* Rutty. Phil. Transſ. N° 406. p. 630.

THE making of tin-plates, or latten, as it is call'd, not being commonly practis'd in *England*, tho' there be so great a conſumption of it, either becauſe the method is not ſufficiently known, or becauſe that in uſe to make ſmall quantities for particular purpoſes is much too dear to anſwer the artificer's expectation in making larger, whereby we are obliged to export our own tin to *Germany*, to receive it back again, manufactured: Dr. *Rutty* therefore has extracted the method the *Germans* themſelves make uſe of, from a diſſertation of M. *De Reaumur*, publiſh'd in the *Memoirs of the Royal Academy of Sciences at Paris*; in which he likewiſe lays down ſome improvements, as he thinks, of his own.

He takes notice then that the making of tin-plates (which in *France* is call'd *white-iron)* does not properly begin; till they go about to prepare the leaves, or plates of iron, that are to be tinn'd; which are ſuppos'd to be ſufficiently thin and flat, and cut into ſquares: But there are only certain ſorts

of iron which can be reduced into these leaves, of which such are the most proper, as when heated, are easiest extendible, and yet can be forged with a hammer when cold; the more soft and exceeding flexible, as well as the more brittle being to be rejected.

These leaves are drawn from bars of iron, about an inch square; which being made a little flat, they cut into thin pieces or soles (*semelles*) which they fold together; and having made them into parcels, containing 40 leaves each, batter them all at once with a hammer, that weighs from 600 to 700 *lb*.

After this the principal part of the whole art is to prepare these leaves: For, the lightest dust, or least rust upon their surface, will hinder the tin from uniting with them. This may, it is true, be taken off by filing, but that being much too expensive, the same may be brought about by steeping the plates (to what number you please) in acid waters, for a certain time; and when they are taken out, scouring them with sand, in order to fetch off any thing that may remain on the surface: And by this method a woman may clean more plates in an hour, than the most expeditious workman can file in several days.

Of these waters, the author mentions several: But what the *Germans* themselves us'd (and which they make a mighty secret of) he found to be only common water, made eager with rye, which requires very little pains: For, after they have ground the grain grossly, and pounded it, they leave it to ferment in common water for a certain time, and with a little patience they are sure to have an eager *menstruum*. With this *menstruum* they fill troughs, or tuns, into which they put piles of iron-plates; and to make it grow eager the better, and have more activity, they keep these vessels in vaults, or stoves, which have little air, and in which they keep lighted charcoal. The workmen go into these vaults once or twice a day, either to turn the plates, that they may be equally expos'd to the action of the acid liquor, or to take out those that are sufficiently cleans'd, or put others in the room: And as the liquor is more acid, or the heat of the vault, or stove more intense, the plates are sooner cleans'd; but it requires at least two days, and sometimes a great deal more.

This is the method, which the *Germans* employ'd in the tin works in *France*, constantly made use of to prepare the iron-

iron-plates to receive the coat of tin: But as the author observ'd, that the constant attendance on them in the stove was very laborious, the heat therein being almost insupportable to those who are not us'd to it; he proposes some other methods, which are attended with very little trouble, and as small, if not a less expence; and which upon trial succeeded full as well.

Having, therefore, observ'd, that the iron leaves, or plates, are covered with a scale, or layer, half vitrified by the fire, on which acids have none, or very little effect, he imagined, that instead of dissolving the iron in these acid waters, it would be better to make it rust, and thereby put it in a condition to be easier cleans'd from those scales; as rust is accompanied with a kind of fermentation, and rarefaction, and the matter, which rusts, takes up a greater space, and raises up whatever opposes it. To this purpose he steep'd iron-plates in different acid *menstruum's*, as in water in which allum, common salt, and *sal-armoniac* were separately dissolv'd; and others of the same iron he only dipp'd into the same waters, and instantly taking them out, expos'd them to the air: These latter were rusted by all of them, but sooner by that in which the *sal-armoniac* was dissolv'd. After two days, during which every plate had been dipp'd into the *menstruum* but twice or thrice, he scour'd them, and likewise those he had left to steep for that time; and comparing them together, found that those, which had only been wetted at different times, cleans'd better than those, which were steep'd; the rust covering all the surface of the latter, without raising the scale; whereas in the former, as soon as one part of the metal is detach'd, it is attracted by the *menstruum*, and the surface is rais'd into blisters of rust.

These dissolvents, the author takes notice, tho' weak in themselves, yet produce the effect as well as the stronger, which are much dearer: But amongst the latter he prefers vinegar, which being very plentiful in *France* may be made use of with little cost: For, you need only dip each leaf into it, and immediately take it out again; leaving it afterwards in some moist place, and it will be scaled in 48 hours; if you take care to repeat this three or four times in a day. The scaling will still be more expeditious, if you dissolve a little *sal-armoniac* in the vinegar; a pound or two to a puncheon: For, as vinegar dissolves iron well; so *sal-armoniac*, as has been just observ'd, rusts it sooner than any other salt: But

this

this muſt be us'd very moderately, and the leaf be left to ſteep in clean water to diſſolve any particles of it, that may ſtick to its ſurface, which may otherwiſe make it ruſt after it has been tinn'd.

If you ſcale with vinegar, and want to do it at a leſs expence, you need only plunge the leaves once or twice at fartheſt; and when the vinegar is dried on the ſurface, ſprinkle it with water; or dip them into it, and take them out immediately.

There are ſeveral other ways of making iron ruſt; as keeping it in a moiſt cellar, expoſing it to the dew, ſprinkling it with ſimple water, ſeveral times in a day, which, by diſſolving *ſal-armoniac* therein, will ſtill act quicker. In thoſe countries where the pyrites is common, the vitriolic waters will ſcale them ſoon enough, which are almoſt as cheap as common water: You need only heap the pyrites together, and leaving them to moulder in the air, make afterwards a *lixivium* with them and common water, which lye will have the deſir'd effect: But as the plates of iron are ſenſibly much eaſier cleans'd on one ſide than the other, the bad ſide rarely taking the brilliant poliſh in the tinning, but having always ſome ſpots; which is owing to this, namely, that in the battering, one ſide is more expos'd to the action of the hammer, and is therefore better plain'd, the author again adviſes not to ſteep them, but only to moiſten them, in order to make them ruſt, whereby you need moiſten that ſide only which wants it moſt: Whereas if you ſteep them, as the bad ſide will take double, or triple the time of the other, the acid *menſtruum* will diſſolve the ſurface, and occaſion a loſs of iron.

He next gives two cautions neceſſary to be followed: The firſt is in the management of the plates before they come to be prepar'd, which is in the battering of them, to change the place of each in its turn, that every one may receive the immediate action of the hammer, otherwiſe they will not extend equally: The ſecond is to ſteep them in clay, or fullers-earth, temper'd with water before you heat them, to prevent their ſoldering with one another.

He then cloſes this part of the operation with remarking, that whichſoever of theſe methods are pitch'd upon; whether the old one, of which he has learn'd the ſecret; or any of the new, which he has here ſhewn, it is abſolutely neceſſary after the plates are ſufficiently ſcal'd, to ſcour them with ſand;

and when there remain no more black ſpots on their ſurface, to throw them into water to prevent their ruſting again, and leave them therein till the inſtant you would tin them, or as it is call'd, blanche them. This he obſerves is the very object of the whole art, and kept as much a ſecret by the blancher, as the acid corroding *menſtruum* is by the ſcaler. But the manner of doing it is thus.

They flux the tin in a large iron crucible, of the figure of a broken pyramid with four faces, of which the two oppoſite ones are leſs than the two others: This crucible they heat only from below, its upper border being luted in the furnace quite round: The crucible is always deeper than the plates which are to be tinn'd are long; which they always put in downright, and the tin ought to ſwim over them. For this purpoſe artificers of different trades prepare the plates in different manners, which are all exceptionable: But the *Germans* he perceiv'd made uſe of no preparation whatſoever, only putting the ſcour'd plates into clean water, as has been remark'd; but when the tin is melted in the crucible, they cover it with a layer of a ſort of ſuet, an inch or two thick, thro' which the plate muſt paſs before it come to the tin: The firſt uſe of this is to keep the tin from burning; and if any part ſhould take fire, as the ſuet will ſoon moiſten it, to reduce it to its natural ſtate again. This ſuet is compounded, as the blanchers ſay, and is of a black colour, which the author thought might be given it with ſoot, or the ſmoke of a chimney, only to ſpread a myſtery over their work: But he found it true ſo far, that common unprepar'd ſuet was not ſufficient: For, after ſeveral attempts, there was always ſomething wanting to render the ſucceſs of the operation certain.

The whole ſecret then of blanching lies entirely in the preparation of this ſuet; and this he at laſt diſcover'd to conſiſt only in firſt frying, and burning it; which not only gives it the colour, but puts it into a condition to give the iron a diſpoſition, to be tinn'd, which it does ſurpriſingly.

The tin itſelf ought to have a certain degree of heat: For, if it is not hot enough, it will not ſtick to the iron; if it be too hot, it will cover it with too thin a coat, and the plates will have ſeveral colours, as a mixture of red, blue and yellow; and the whole appear of a dirty yellow caſt. To prevent this by knowing when the tin has a proper degree of heat, they firſt

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make an aſſay with ſmall pieces of the ſcal'd plates, and learn from them, when the tin is in proper order: But generally ſpeaking, they dip the plates into tin that is more or leſs hot, according to the thickneſs they would have the coat to be of. Some plates they only give one layer to, and theſe they plunge into tin, that has a leſſer degree of heat than that into which they plunge thoſe plates, which they would have take two layers; as alſo when they give theſe the ſecond layer, they put them into tin, that has not ſo great a degree of heat, as that into which they were put the firſt time: Beſides, it is to be obſerved, that the tin which is to give the ſecond coat, ought to be freſh covered with ſuet, but only with the common ſort without preparation: For, melted tin is ſufficiently diſpoſed to attach itſelf to ſolid tin; and in this caſe it is to tin itſelf, to which the new tin is to be joined.

A New apparent Motion diſcovered in the fixed Stars, *its Cauſe aſſigned, the velocity and equable Motion of* Light *deduced; by Mr.* Bradley. Phil. Tranſ. N° 406. p. 637.

THE following obſervations were begun by Mr. *Samuel Molyneux* at *Kew*, continued, and repeated by Mr. *Bradley* at *Kew* and *Wanſtead*, in hopes of verifying thoſe, that Dr. *Hook* (*vide* an attempt to prove the motion of the earth, from obſervations made by Mr. *Robert Hooke*, Fellow of the *Royal Society*, *London* 1674.) formerly communicated to the public, concerning the parallax of the fixed ſtars. The ſame ſtar, therefore, was made choice of by Mr. *Molyneux*, almoſt the ſame method followed, and his inſtrument conſtructed upon principles nearly the ſame, but greatly exceeding the Dr's in exactneſs; which was chiefly owing to the curious Mr. *George Graham*, to whom the lovers of Aſtronomy are alſo indebted for ſeveral other exact, and well contrived inſtruments.

Mr. *Molyneux's apparatus* was compleated and fitted for obſerving about the end of *November* 1725; and on the third day of *December* following, the bright ſtar in the head of *Draco* (marked γ by *Bayer*) was for the firſt time obſerved, as it paſſed near the zenith, and its ſituation carefully taken with the inſtrument.

The like obſervations were made on the 5, 11, and 12 days of the ſame month; and there appearing no material difference in the place of the ſtar, a farther repetition of them ſeemed needleſs, it being then a ſeaſon of the year, wherein no ſenſible altera-

alteration of parallax in this ſtar could ſoon be expected. It was chiefly, therefore, curioſity that tempted Mr. *Bradley* (being then at *Kew*, where the inſtrument was fixed) to prepare for obſerving the ſtar on *December* 17; when having adjuſted the inſtrument as uſual, he perceived that it paſſed a little more ſoutherly this day than when it was obſerved before. Not ſuſpecting any other cauſe of this appearance, we firſt concluded, that it was owing to the uncertainty of the obſervations; and that either this, or the foregoing, were not ſo exact, as we had before ſuppoſed: For which reaſon we purpoſed to repeat the obſervation again, in order to determine, whence this difference proceeded: And upon doing it *December* 20. Mr. *Bradley* found that the ſtar paſſed ſtill more ſoutherly than in the former obſervations. This ſenſible alteration was the more ſurpriſing, in that it was the contrary way from what it would have been, had it proceeded from an annual parallax of the ſtar: But being now pretty well ſatisfied, that it could not be entirely owing to the want of exactneſs in the obſervations; and having no notion of any thing elſe, that could cauſe ſuch an apparent motion, as this in the ſtar; we began to think that ſome change in the materials, &c. of the inſtrument itſelf, might have occaſioned it. Under theſe apprehenſions we remained ſome time; but being at length fully convinced, by ſeveral trials, of the great exactneſs of the inſtrument, and finding by the gradual increaſe of the ſtar's diſtance from the pole; that there muſt be ſome regular cauſe that produced it; we took care to examine nicely, at the time of each obſervation, how much it was. And about the beginning of *March* 1726, the ſtar was found to be 20″ more ſoutherly than at the time of the firſt obſervation: It now, it is true, ſeemed to have arrived at its utmoſt limit ſouthward; becauſe in ſeveral trials made about this time, no ſenſible difference was obſerved in its ſituation. By the middle of *April* it appeared to be returning back again towards the north; and about the beginning of *June*, it paſſed at the ſame diſtance from the zenith, as it had done in *December*, when it was firſt obſerved.

From the quick alteration of this ſtar's declination about this time (it increaſing 1″ in 3 days) it was concluded, that it would now proceed northwards, as it before had gone ſouthwards of its preſent ſituation; and it happened as was conjectured: For, the ſtar continued to move northwards till *Sept.* following, when it again became ſtationary, being then near 20″ more northerly than in *June*, and no leſs than 39″ more northerly than it was

in *March*. From *September* the ſtar returned towards the ſouth, till it arrived in *December* to the ſame ſituation it was in at that time twelve months, allowing for the difference of declination, on account of the preceſſion of the equinox.

This was a ſufficient proof, that the inſtrument had not been the cauſe of this apparent motion of the ſtar; and to find one adequate to ſuch an effect ſeemed a difficulty: A mutation of the earth's axis was one of the firſt things that offered itſelf upon this occaſion; but it was ſoon found inſufficient: For, tho' it might have accounted for the change of declination in γ *Draconis*; yet it would not at the ſame time agree with the phenomena in other ſtars, particularly in a ſmall one almoſt oppoſite in *R. Aſcenſ.* to γ *Draconis*, at about the ſame diſtance from the north pole of the equator: For, tho' this ſtar ſeemed to move the ſame way, as a nutation of the earth's axis would have made it; yet changing its declination but about half as much as γ *Draconis* in the ſame time (as appeared upon comparing the obſervations of both, made upon the ſame days, at different ſeaſons of the year) this plainly proved, that the apparent motion of the ſtars was not occaſioned by a real nutation; ſince if that had been the cauſe, the alteration in both ſtars would have been near equal.

The great regularity of the obſervations left no room to doubt, but that there was ſome regular cauſe that produced this unexpected motion, which did not depend on the uncertainty, or variety of the ſeaſons of the year. Upon comparing the obſervations with each other, it was found, that in both the abovementioned ſtars, the apparent difference of declination from the *maxima* was always nearly proportional to the verſed ſine of the ſun's diſtance from the equinoctial points. This was an inducement to think, that the cauſe whatever it was, had ſome relation to the ſun's ſituation with reſpect to thoſe points. But not being able to frame any hypotheſis at that time, ſufficient to ſolve all the phenomena; and being very deſirous to ſearch a little farther into this matter, Mr. *Bradley* began to think of erecting an inſtrument for himſelf at *Wanſtead*, that having it always at hand, he might with the more eaſe and certainty, enquire into the laws of this new motion. The conſideration likewiſe of being able by another inſtrument, to confirm the truth of the obſervations, hitherto made with Mr. *Molyneux*'s, was no ſmall inducement; but the chief of all was the opportunity he ſhould thereby have of trying, in what manner other ſtars were affected by the ſame cauſe, whatever it was:

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For, Mr. *Molyneux*'s inſtrument being originally deſigned for obſerving γ *Draconis* (in order, as he ſaid before, to try whether it had any ſenſible parallax) was contrived in ſuch a manner, as to be capable of but little alteration in its direction, not above ſeven, or eight minutes of a degree: And there being few ſtars within half that diſtance from the zenith of *Kew* bright enough to be well obſerved; he could not, with his inſtrument, thoroughly examine, how this cauſe affected ſtars differently ſituated with reſpect to the equinoctial, and ſolſtitial points of the ecliptic.

Theſe conſiderations determined Mr. *Bradley*; and by the contrivance, and direction of Mr. *Graham*, his inſtrument was fixed up, *Auguſt* 19, 1727: As he had no convenient place where he could make uſe of ſo long a teleſcope as Mr. *Molyneux*'s, he contented himſelf with one of but little more than half the length of his (*viz.* of about 12 foot and ½; his being 24 and ½) judging from the experience he had already had that this radius would be long enough to adjuſt the inſtrument to a ſufficient degree of exactneſs; and he had no reaſon since to change his opinion: For, from all the trials he had hitherto made, he is very well ſatisfied, that when it is carefully rectified, its ſituation may be ſecurely depended on to half a ſecond. As the place where his inſtrument was to be hung, in ſome meaſure determined its radius; ſo did it likewiſe the length of the arch, or limb, on which the diviſions were made to adjuſt it: For, the arch could not conveniently be extended farther, than to reach to about 6° and ¼ on each ſide his zenith. This, it is true, was ſufficient: Since it gave him an opportunity of making choice of ſeveral ſtars, very different both in magnitude and ſituation; there being more than 200 inſerted in the *British Catalogue* that may be obſerved with it. Mr. *Bradley* needed not to have extended the limb ſo far; but that he was willing to take in *Capella*, the only ſtar of the firſt magnitude that comes ſo near his zenith.

His inſtrument being fixed, he immediately began to obſerve ſuch ſtars, as he judged moſt proper to give him light into the cauſe of the motion, already mentioned: There was variety enough of ſmall ones; and not leſs than twelve, that he could obſerve thro' all the ſeaſons of the year; they being bright enough to be ſeen in the day-time, when neareſt the ſun. He had not been long obſerving, before he perceived, that the notion he had before entertained of the ſtars being fartheſt north and ſouth, when the ſun was about the equinoxes, was on-

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true of thoſe that were near the ſolſtitial colure: And after Mr. *Bradley* had continued his obſervations a few months, he diſcovered what he then apprehended to be a general law, obſerved by all the ſtars, *viz.* that each of them became ſtationary, or was fartheſt north or ſouth, when they paſſed over his zenith at ſix o'clock, either in the morning or evening. He likewiſe perceived, that whatever ſituation the ſtars were in with reſpect to the cardinal points of the ecliptic, the apparent motion of every one tended the ſame way, when they paſſed his inſtrument about the ſame hour of the day or night: For, they all moved ſouthwards, while they paſſed in the day, and northwards in the night; ſo that each was fartheſt north, when it came about ſix o'clock in the evening, and fartheſt ſouth, when it came about ſix o'clock in the morning.

Tho' he ſince diſcovered, that the *maxima* in moſt of theſe ſtars do not happen exactly, when they come to his inſtrument at thoſe hours; yet not being able at that time to prove the contrary; and ſuppoſing that they did, he endeavoured to find out what proportion the greateſt alterations of declination in different ſtars bore to each other; it being very evident that they did not all change their declination equally: He has before taken notice, that it appeared from Mr. *Molyneux*'s obſervations, that γ *Draconis* altered its declination about twice as much as the afore-mentioned ſmall ſtar, almoſt oppoſite to it; but examining the matter more particularly, he found that the greateſt alteration of declination in theſe ſtars was, as the ſine of the latitude of each reſpectively. This made him ſuſpect that there might be the like proportion between the *maxima* of other ſtars; but finding that the obſervations of ſome of them would not perfectly correſpond with ſuch an hypotheſis; and not knowing, whether the ſmall difference he met with, might not be owing to the uncertainty, or error of the obſervations, he deferred the farther examination into the truth of this hypotheſis, till he ſhould be furniſhed with a ſeries of obſervations, made in all parts of the world; which might enable him, not only to determine what errors the obſervations are liable to, or how far they may ſafely be depended upon, but alſo to judge, whether there had been any ſenſible change in the parts of the inſtrument itſelf.

Upon theſe conſiderations, he laid aſide all thoughts at that time about the cauſe of the forementioned phenomena, hoping that he ſhould the more eaſily diſcover it, when he was better pro-

provided with proper means to determine more precisely what they were.

When the year was compleated, he began to examine and compare his observations; and having pretty well satisfied himself as to the general laws of the phenomena, he then endeavoured to find out the cause of them. He was already convinced, that the apparent motion of the stars was not owing to a nutation of the earth's axis. The next thing that offered itself, was an alteration in the direction of the plumb-line, with which the instrument was constantly rectified; but this upon trial proved insufficient. Then he considered what refraction might do; but here likewise nothing satisfactory occurred. At last he conjectured, that all the phenomena hitherto mentioned, proceeded from the progressive motion of light, and the earth's annual motion in its orbit: For, he perceived, that if light were propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest, as when it is moving in any other direction, than that of the line passing thro' the eye and object; and that when the eye is moving in different directions, the apparent place of the object would be different.

Mr. *Bradley* considered this matter in the following manner: He imagined C A (Fig. 23. Plate XI.) to be a ray of light falling perpendicularly upon the line B D; then if the eye be at rest at A, the object must appear in the direction A C, whether light be propagated in time, or in an instant. But if the eye be moving from B towards A, and light be propagated in time, with a velocity that is to the velocity of the eye, as C A to B A; then light moving from C to A, whilst the eye moves from B to A, that particle of it, by which the object will be discerned, when the eye in its motion comes to A, is at C, when the eye is at B: Joining the points B, C, he supposed the line B C to be a tube (inclined to the line B D in the angle D B C) of such a diameter, as to admit of but one particle of light; then it was easy to conceive, that the particle of light at C (by which the object must be seen, when the eye, as it moves along, arrives at A) would pass thro' the tube B C, if it be inclined to B D in the angle D B C, and accompanies the eye in its motion from B to A; and that it could not come to the eye, placed behind such a tube, if it had any other inclination to the line B D. If instead of supposing C B so small a tube, we imagine it to be the axis of a larger; then for the same reason the particle

ticle of light at C could not pass thro' that axis, unless it be inclined to BD, in the angle CBD. In like manner if the eye moved the contrary way from D towards A, with the same velocity; then the tube must be inclined in the angle BDC. Tho', therefore, the true or real place of an object be perpendicular to the line in which the eye is moving; yet the visible place will not be so; since that, no doubt, must be in the direction of the tube: But the difference between the true and apparent place will be *(cæteris paribus)* more or less, according to the different proportion between the velocity of light, and that of the eye. So that if we could suppose, that light is propagated in an instant; then there would be no difference between the real and visible place of an object, tho' the eye were in motion: For, in that case AC being infinite with respect to AB, the angle ACB (the difference between the true and visible place) vanishes. But if light be propagated in time (which he presumes will readily be allowed by most of the philosophers of this age) then it is evident from the foregoing considerations, that there will be always a difference between the real and visible place of an object; unless the eye be moving, either directly towards, or from the object. And in all cases, the sine of the difference between the real and visible place of the objects will be to the sine of the visible inclination of the object to the line, in which the eye is moving, as the velocity of the eye to that of light.

If light moved but 1000 times faster than the eye, and an object (supposed to be at an infinite distance) was really placed perpendicularly over the plane in which the eye is moving; it follows from what has been already said, that the apparent place of such an object will be always inclined to that plane, in an angle of 89° 56′ and ½: So that it will constantly appear 3′ and ½ from its true place, and seem so much less inclined to the plane, that way towards which the eye tends: That is, if AC be to AB (or AD) as 1000 to 1; the angle ABC will be 89° 56′ and ½, and ACB = 3′ and ½, and BCD = 2 ACB = 7′. So that according to this supposition, the visible or apparent place of the object will be altered 7′, if the direction of the motion of the eye be at one time contrary to what it is at another.

If the earth revolve round the sun annually and the velocity of light were to that of the earth's motion in its orbit (which Mr. *Bradley* at present supposes to be a circle) as 1000 to 1; then it is easy to conceive, that a star really placed in the very

pole of the ecliptic, would, to an eye carried along with the earth, ſeem to change its place continually, and (neglecting the ſmall difference on account of the earth's diurnal revolution on its axis) would ſeem to deſcribe a circle round that pole, every way diſtant therefrom 3′ and $\frac{1}{2}$. So that its longitude would be varied thro' all the points, of the ecliptic every year; but its latitude always remain the ſame. Its right aſcenſion and its declination would likewiſe change, according to the different ſituation of the ſun in reſpect to the equinoctial points; and its apparent diſtance from the north pole of the equator would be 7′ leſs at the autumnal, than at the vernal equinox.

The greateſt alteration of the place of a ſtar in the pole of the ecliptic (or which in effect amounts to the ſame thing, the proportion between the velocity of light and the motion of the earth in its orbit) being known, it will not be difficult to find what would be the difference, upon this account, between the true and apparent place of any other ſtar at any time; and on the contrary, the difference between the true and apparent place being given, the proportion of the velocity of light, and the motion of the earth in its orbit may be found.

As Mr. *Bradley* only obſerved the difference of declination of the ſtars, he does not now take any farther notice in what manner ſuch a cauſe, as he has here ſuppoſed, would occaſion an alteration in their apparent places in other reſpects; but ſuppoſing the earth to move equally in a circle, it may be gathered from what has been already ſaid, that a ſtar, which is neither in the pole, nor plane of the ecliptic, will ſeem to deſcribe about its true place a figure, inſenſibly different from an ellipſe, whoſe tranſverſe axis is at a right angle to the circle of longitude, paſſing thro' the ſtars true place, and equal to the diameter of the little circle, deſcribed by a ſtar (as was before ſuppoſed) in the pole of the ecliptic; and whoſe conjugate axis is to its tranſverſe axis, as the ſine of the ſtars latitude to the radius. And allowing that a ſtar by its apparent motion does exactly deſcribe ſuch an ellipſis; it will be found, that if A be the angle of poſition (or the angle at the ſtar, formed by two great circles, drawn from it, thro' the poles of the ecliptic and equator) and B be another angle, whoſe tangent is to that of A, as radius to the ſine of the latitude of the ſtar; then B will be equal to the difference of longitude between the ſun and the ſtar, when the true and apparent declination of the ſtar are the ſame: And if the ſun's longitude in the ecliptic be reckoned from that point, in which it is when this happens; then the diffe-

difference between the true, and apparent declination of the ſtar (on account of the cauſe Mr. *Bradley* is now conſidering) will be always as the ſine of the ſun's longitude from thence: It will likewiſe be found, that the greateſt difference of declination, that can be between the true and apparent place of the ſtar, will be to the ſemi-tranſverſe axis of the ellipſis (or to the ſemi-diameter of the little circle, deſcribed by a ſtar in the pole of the ecliptic) as the ſine of A to the ſine of B.

If the ſtar have north latitude; the time, when its true and apparent declination is the ſame, is, before the ſun comes in conjunction with, or oppoſition to it, if its longitude be in the firſt or laſt quadrant (*viz.* in the aſcending ſemi-circle) of the ecliptic; and after them, if in the deſcending ſemi-circle; and it will appear neareſt to the north pole of the equator, at the time of that *maximum* (or when the greateſt difference between the true and apparent declination happens) which preceeds the ſun's conjunction with the ſtar.

Mr. *Bradley* purpoſely omitted ſome matters of no great moment; and conſidered the earth as moving in a circle and not an ellipſis, to avoid too perplex'd a *calculus*, which after all the trouble of it would not ſenſibly differ from that he makes uſe of; eſpecially in thoſe conſequences, which he at preſent draws from the foregoing hypotheſis.

This being premiſed, he now proceeds to determine from the obſervations, what the real proportion is between the velocity of light and that of the earth's annual motion in its orbit; upon ſuppoſition that the phenomena, beforementioned, depend upon the cauſes here aſſigned: But firſt he lets us know, that in all the obſervations hereafter mentioned, he has made an allowance for the change of the ſtar's declination on account of the preceſſion of the equinox; upon ſuppoſition that the alteration from this cauſe is proportional to the time, and regular throughout all the parts of the year. He has deduced the real annual alteration of declination of each ſtar from the obſervations themſelves; and he the rather chooſes to depend upon them in this article; becauſe all he has hitherto made, concur to prove, that the ſtars, near the equinoctial colure, change their declination at this time 1″ and ½ or 2″ in a year more than they would do, if the preceſſion was only 50″, as is now generally ſuppoſed. He has likewiſe met with ſome ſmall varieties in the declination of other ſtars in different years, which do not ſeem to be owing to the ſame cauſe; particularly, in thoſe that are near the ſolſtitial colure; which on the contrary have altered their

their declination less than they ought, if the precession were 50″. But whether these small alterations proceed from a regular cause, or are occasioned by any change in the materials, &c. of his instrument, Mr. *Bradley* was not hitherto able fully to determine: However, he thought it might not be amiss just to mention, how he endeavoured to allow for them; tho' the result would have been nearly the same, if he had not considered them at all. What that is, he shews, first from the observations of γ *Draconis*, which was found to be 39″ more southerly in the beginning of *March*, than in *September*.

From what hath been premised it will appear, that the greatest alteration of the apparent declination of γ *Draconis*, on account of the successive propagation of light, would be to the diameter of the little circle which a star (as was before remarked) would seem to describe about the pole of the ecliptic as 39″ to 40′,4. The half of this is the angle A C B (as represented in the Fig.) This therefore, being 20″,2; A C will be to A B, that is, the velocity of light to that of the eye (which in this case may be supposed the same, as the velocity of the earth's annual motion in its orbit) as 10210 to 1: Whence it would follow, that light moves, or is propagated as far as from the sun to the earth in 8′ 12″.

It is well known, that M. *Romer*, who first attempted to account for an apparent inequality in the times of the eclipses of *Jupiter*'s satellites, by the hypothesis of the progressive motion of light, supposed, that it spent about 11 minutes of time in its passage from the sun to us: But it hath since been concluded by others from the late eclipses, that it is propagated as far in about seven minutes. The velocity of light, therefore, deduced from the foregoing hypothesis, is a mean, as it were, between what had at different times been determined from the eclipses of *Jupiter*'s satellites.

These different methods of finding the velocity of light thus agreeing in the result, we may reasonably conclude, not only that these phenomena are owing to the causes to which they have been ascribed; but also that light is propagated (in the same medium) with the same velocity after it hath been reflected as before: For, this will be the consequence, if we allow that the light of the sun is propagated with the same velocity, before it is reflected, as the light of the fixed stars. And Mr. *Bradley* imagines, this will scarce be questioned; if it can be made appear, that the velocity of the light of all the fixed stars is equal, and that their light moves, or is propagated thro' equal

spaces

ſpaces in equal times, at all diſtances from them: Both which points (as Mr. *Bradley* apprehends) are ſufficiently prov'd from the apparent alteration of the declination of ſtars of different luſtre; for, that is not ſenſibly different in ſuch ſtars as ſeem near one another; tho' they appear of very different magnitudes: And whatever their ſituations are (if he proceed according to the foregoing hypotheſis) he finds the ſame velocity of light from his obſervations of ſmall ſtars of the fifth or ſixth, as from thoſe of the ſecond and third magnitude, which in all probability are placed at very different diſtances from us. The ſmall ſtar, for inſtance, before ſpoken of, that is almoſt oppoſite to γ *Draconis* (being the 35th *Camelopard. Hevelii* in Mr. *Flamſtead*'s catalogue) was 19″ more northerly about the beginning of *March* than in *Sept.* whence Mr. *Bradley* concludes, according to his hypotheſis, that the diameter of the little circle, deſcribed by a ſtar in the pole of the ecliptic, would be 40″,2.

The laſt ſtar of the great bear's tail, of the ſecond magnitude (mark'd η by *Bayer*) was 36″ more ſoutherly about the middle of *January* than in *July*. Hence the *maximum* or greateſt alteration of declination of a ſtar in the pole of the ecliptic would be 40″, 4, exactly the ſame as was before found from the obſervations of γ *Draconis*.

The ſtar of the fifth magnitude in the head of *Perſeus*, mark'd τ by *Bayer*, was 25″ more northerly about the end of *December* than on the 29th of *July* following: Hence the *maximum* would be 41″. This ſtar is not bright enough to be ſeen, as it paſſes over Mr. *Bradley*'s zenith about the end of *June*, when it ſhould be, according to the hypotheſis, fartheſt ſouth: But becauſe he can more certainly depend upon the greateſt alteration of declination of thoſe ſtars, which he has frequently obſerv'd about the times, when they become ſtationary, with reſpect to the motion he is now conſidering, he ſets down a few more inſtances, of ſuch, from which we may be able to judge how near it may be poſſible from theſe obſervations to determine with what velocity light is propagated.

Bayer's α *Perſei* was 23″ more northerly at the beginning of *January* than in *July*: Hence the *maximum* would be 40″, 2.

α *Caſſiopeiæ* was 34″ more northerly about the end of *December* than in *June*: Hence the *maximum* would be 40″, 8.

β *Dra-*

β *Draconis* was 39″ more northerly in the beginning o
September than in *March*: Hence the *maximum* would b
40″, 2.

Capella was about 16″ more southerly in *August* than i
September: Hence the *maximum* would be abour 40″: Bu
this star being farther from Mr. *Bradley*'s zenith than thos
he before made use of, he cannot so well depend upon hi
observations of it, as of the others; because he meets wit
some small altaretions of its declination, that do not seem t
proceed from the cause he is now considering.

He compared the observations of several other stars, an
they all conspire to prove, that the *maximum* is about 40″ o
41″; he, therefore, supposes, that it is 40″ ½ or (whic
amounts to the same thing) that light moves, or is propagate
as far as from the sun to us in 8′ 13″. The near agreemen
he met with in his observations induces him to think, tha
the *maximum* (as he has here fix'd it) cannot differ so much a
a second from the truth; and therefore, it is probable, tha
the time, which light spends in passing from the sun to u
may be determined by these observations within 5″ or 10″
which is such a degree of exactness, as we can never hop
to attain from the eclipses of *Jupiter*'s satellites.

Having thus found the *maximum*, or what the greatest alte
ration of declination would be in a star, placed in the pol
of the ecliptic; he now deduces from it (according to th
foregoing hypothesis) the alteration of declination in one o
two stars, at such times as they were actually observ'd, i
order to see how the hypothesis will correspond with th
phenomena through all the parts of the year.

He thinks it would be too tedious to set down the whol
series of his observations; he therefore, makes choice only o
such as are most proper for his present purpose, and begin
with those of γ *Draconis*.

This star appear'd farthest north about *September* 7, 172
as it ought to have done according to his hypothesis. Th
following table shews how much more southerly the star wa
found to be by observation in several parts of the year; an
likewise how much more southerly it ought to be accordin
to the hypothesis.

1727	D	The difference of declination by observation	The difference of declination by the hypothesis.
		″	″
October	20	4 ½	4 ½
November	17	11 ½	12
December	6	17 ½	18 ½
———	28	25	26
1728			
January	24	34	34
February	10	38	37
March	7	39	39
———	24	37	38
April	6	36	36 ½
May	6	28 ½	29 ½
June	5	18 ½	20
———	15	17 ½	17
July	3	11 ½	11 ½
August	2	4	4
September	6	0	0

Hence it appears, that the hypothesis corresponds with the bservations of this star thro' all parts of the year: For, the mall differences between them seem to arise from the uncerinty of the observations; which is occasioned (as Mr. *Bradley* magines) by the tremulous or undulating motion of the air nd of the vapours therein; which causes the stars sometimes dance to and fro, so much that it is difficult to judge, when ney are exactly on the middle of the wire, that is fix'd in the ommon focus of the glasses of the telescope.

Mr. *Bradley* acknowledges, that the agreement of the bservations with each other, as well as with the hypothesis, much greater than he expected to find, before he had comr'd them; and it may possibly be thought to be too great those who have been us'd to astronomical observations, and now how difficult it is to make such as are in all respects xact: But if it would be any satisfaction to such persons ill Mr. *Bradley* has an opportunity of describing his instruent, and his manner of using it) he can assure them, that upwards of 70 observations he made of this star in a year,

there is but one (and that is noted as very dubious on accoun of clouds) which differs from the foregoing hypothesi more than 2″; and this does not differ 3″.

This, therefore, being the fact, Mr. *Bradley* cannot bu think it very probable, that the phenomena proceed fro the cause he has assigned; since the fore-going observation make it sufficiently evident, that the effect of the real caus whatever it is, varies in this star, in the same proportion th it ought according to the hypothesis.

But lest γ *Draconis* may not be thought so proper to she the proportion, in which the apparent alteration of declinatio is increas'd, or diminish'd, as those stars which lie near th equinoctial colure; Mr. *Bradley* likewise gives the compari son between the hypothesis, and the observations of η of *Ur major*, which was farthest south about the 17. of *Januar* 1728, agreeable to the hypothesis.

The following table shews how much more northerly was found by observation in several parts of the year, an also what the difference should have been according to th hypothesis.

1727	D	Difference of declination by observation. ″	The difference of declination by the hypothesis. ″
September	14	29 ½	28 ½
———	24	24 ½	25 ½
October	16	19 ½	19 ½
November	11	11 ½	10 ½
December	14	4	3
1728			
February	17	2	3
March	21	11 ½	10 ½
April	16	18 ½	18
May	5	24 ½	23 ½
June	5	32	31 ½
———	25	35	34 ½
July	17	36	36
August	2	35	35 ½
September	20	26 ½	36 ½

Mr. *Bradley* finds upon examination, that the hypothesis agrees altogether as exactly with the observations of this star, as the former: For, in about 50 that were made of it in a year, he does not meet with a difference of so much as 2″; except in one, which is mark'd as doubtful on account of the undulation of the air, &c. And this does not differ 3″ from the hypothesis.

The agreement between the hypothesis and the observations of this star is the more to be regarded; since it proves, that the alteration of declination, on accouut of the precession of the equinox, is, (as he before suppos'd) regular thro' all parts of the year: so far at least as not to occasion a difference great enough to be discovered with this instrument: It likewise proves the other part of his former suppositions, *viz.* that the annual alteration of declination in stars near the equinoctial colure, is at this time greater than a precession of 50″ would occasion: For, this star was 20″ more southerly in *September* 1728, than in *September* 1727, that is, about 2″ more than it would have been, if the precession were but 50″: But he may hereafter, perhaps, be better able to determine this point, from his observations of those stars that lie near the equinoctial colure, at about the same distance from the north pole of the equator, and nearly opposite in right ascension.

Mr. *Bradley* thinks it needless to give the comparison between the hypothesis and the observations of any more stars; since the agreement in the foregoing is a kind of demonstration (whether it be allow'd that he has discovered the real cause of the phenomena or not) that the hypothesis gives at least the true law of the variation of declination in different stars, with respect to their different situations and aspects as to the sun: And if this be the case, it must be granted, that the parallax of the fixed stars is much smaller than hath been hitherto suppos'd by those who have pretended to deduce it from their observations. Mr. *Bradley* believes, he may venture to affirm, that in either of the two last mentioned stars, it does not amount to 2″. He is of opinion, that if it were 1″, he should have perceiv'd it, in the great number of observations he made, especially of γ *Draconis*; which agreeing with the hypothesis (without allowing any thing for parallax) nearly as well when the sun was in conjunction with, as in opposition to this star, it seems very probable, that its parallax is not so great as one single second;

and consequently, that it is upwards of 400000 times farther from us than the sun.

As to Dr. *Hook*'s observations, Mr. *Bradley* acknowledges, that before Mr. *Molyneux*'s instrument was erected, he had no small opinion of their correctness, the length of his telescope, and the care he pretends to have taken in making them exact, having been strong inducements with him to think them so. And since he has been convinced both from Mr. *Molyneux*'s observations and his own, that the Dr's are really very far from being either exact or agreeable to the phenomena; he is greatly at a loss how to account for it. He cannot well conceive that an instrument the length of 36 feet, constructed in the manner he describes his, could have been liable to an error of near 30″ (which was doubtless the case) if rectified with so much care as he represents.

Mr. *Flamstead*'s observations of the different distances of the pole star from the pole at different times of the year, which thro' mistake were look'd upon by some as a proof of its annual parallax, seem to have been made with much greater care than those of Dr. *Hook*'s: For, tho' they do not all exactly correspond with each other; yet from the whole Mr. *Flamstead* concluded, that the star was 35″, 40″, or 45″, nearer the pole in *December* than in *May* or *July*. And according to Mr. *Bradley*'s hypothesis, it ought to appear 40″ nearer in *December* than in *June*: The agreement, therefore, of the observations with the hypothesis is much greater than could be reasonably expected, considering the radius of the instrument, and the manner in which it was constructed.

An Attempt to solve the Phenomenon *of the Rise of* Vapours, *Formation of* Clouds, *and Descent of* Rain, *by Dr.* Desaguliers. Phil. Trans. N° 407. p. 6.

DR. *Niewentyt*, and some others affirm — that particles of fire, separated from the sun-beams, by adhering to particles of water, make up *moleculæ*, or small bodies, specifically lighter than air; which, therefore, by hydrostatical laws, must rise and form clouds, that remain suspended, when they are rais'd to such a height, that the air about them is of the same specific gravity with themselves.

That rain is produced by the separation of the particles of fire from those of water, which last being then restored to their former specific gravity, can no longer be sustain'd by the

the air but must fall in drops. *Vide Niewentyt's Religious Philosopher, Contemplation* 19. *from Sect.* 13. *to* 25.

Now this is liable to several objections: 1. It is built upon a supposition that fire is a particular substance, or distinct element, which has never hitherto been proved by convincing experiments and sufficient observations; and which Mr. *Hales*, in his excellent book of *vegetable Statics*, has shewn to be an ill-grounded opinion, making it very plain, that in chemical operations, those bodies which had been thought to become heavier by particles of fire adhering to them; were only so by adhesion of particles of air, &c. which he has shewn to be absorbed in considerable quantities, by some bodies, whilst it is generated (or reduced from a fixt to an elastic state) by others; nay, that it may be absorbed and generated successively by the same body, under different circumstances.

2. If we should allow the above-mentioned supposition, the difficulty will still remain about the production of rain by the separation of the fire from the water. For, Dr. *Niewentyt* ascribes this effect to two different causes. 1. To condensation *Sect.* 23. affirming, 'that when contrary winds blow against the 'same cloud, and drive the watery particles together, the fire 'that adhered to them gets loose; and they (becoming then 'specifically heavier) precipitate and fall down in rain.' Then in the very next *section*, he ascribes it to rarifaction, when he affirms, 'that when a wind blowing obliquely upwards causes 'a cloud to rise into a thinner air (*i. e.* specifically lighter than 'itself) the fire, which by adhering to the particles of water 'rendered them lighter, extricates itself from them, and as'cending by its lightness, the water will become too heavy, 'not only to remain in this thin and light air, but even in a 'thicker and heavier near the earth; and so will be turned into 'a descending dew, mist, or rain, or snow, or the like, ac'cording as the watery vapours are either rarified or com'pressed.'

The first of these causes of rain is contrary to experience: For, when two contrary winds blow against each other over any place of the earth, the barometer always rises, and we have fair weather. For then (as Dr. *Halley* says *Phil. Trans.* N° 183.) the air, being accumulated above, becomes specifically heavier about the clouds, which (instead of falling into rain, as Dr. *Niewentyt* supposes) ascend up into such a part of the atmosphere, as has the air of the same specific gravity with themselves.

If

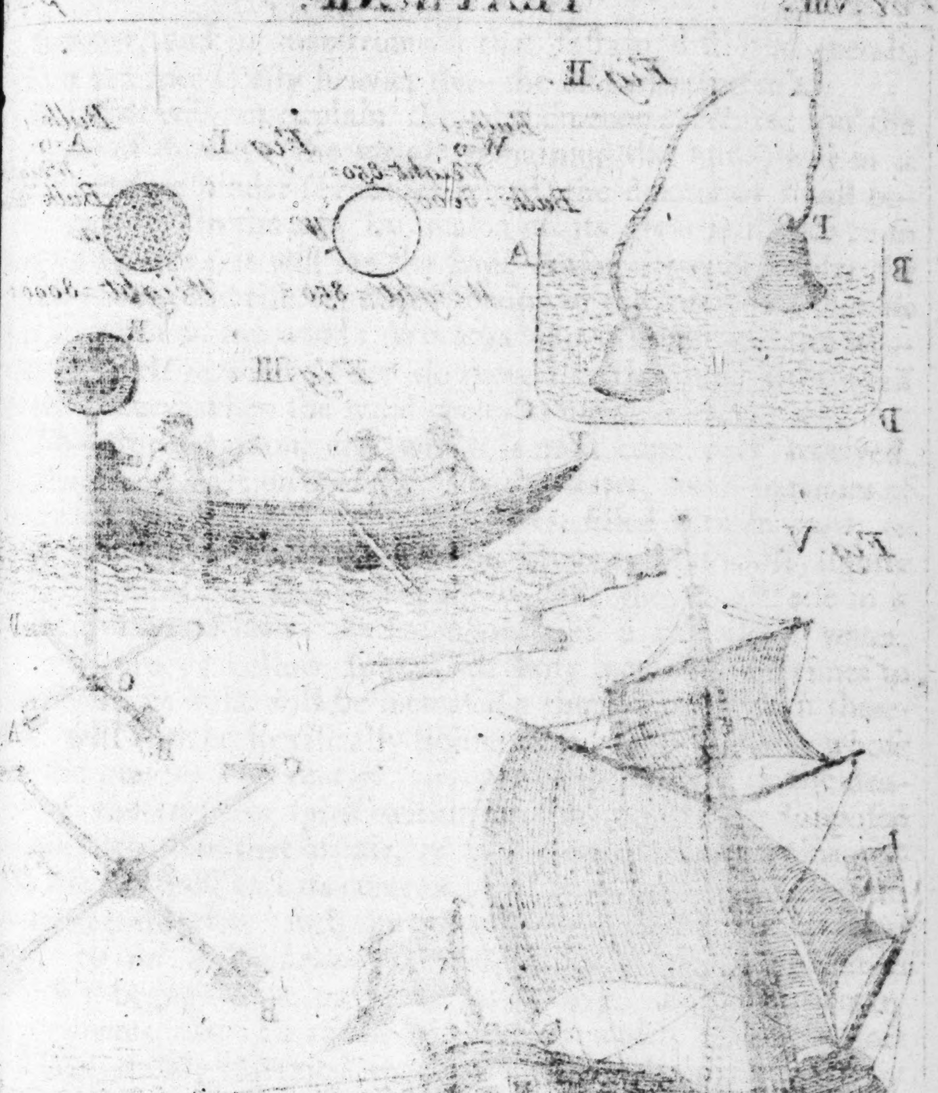
If the falling of rain might be attributed to the second of these causes; then every time a cloud is encompassed with air specifically lighter than itself (whether it be when by the blowing away some of the superior air, that which is about the cloud becomes rarer, as it is less compressed, or by the cloud being driven upwards) rain must necessarily follow: Whereas one may often observe the clouds rise and fall without rain; even when the barometer shews the weight of the air to be altered: For, that happens only, when by the great diminution of the specific gravity of the air about the cloud, it has a great way to fall; in which case the resistence of the air, which increases, as the square of the velocity of the descending cloud, causes the floating particles of water to come within the power of each other's attraction, and form such large drops, as being specifically heavier than any air must fall in rain.

No gentle descent of a cloud, but only an accelerated motion downwards produces rain.

N. B. Dr. *Desaguliers* does not mean that the quick descent of a cloud is the only cause of rain; because the shock from a flash of lightning, and the sudden return of the air, after the *vacuum* made by the flash, will condense the floating vapour into water; and likewise the same cloud, which in the free air might be carried horizontally without being turned into rain, meeting with a high hill in its way, will be condensed and fall into drops; especially, if in the day time it be driven by the wind out of the sun-shine, against the shaded side of the mountain.

Besides all this, if particles of fire were joined with those of water to raise the latter up, those igneous particles must be at least 1000 times larger in bulk than the watery ones: So that a person, who at the top of a hill, has his hands and face in a cloud, must feel a very sensible warmth, by touching a much larger surface of fire than water in the cloud; and afterwards find the rain, produced from that vapour, sensibly colder: Whereas the contrary is proved by our senses; the tops of hills, tho' in the clouds, being much colder than the rain at bottom.

There is another opinion concerning the rise of vapours, namely, that tho' water be specifically heavier than air; yet if its surface be increased by very much diminishing the bulk of its particles; when once raised, it cannot easily fall; because the weight of each particle diminishes as the cube root of its diameter; and the surface to which the air resists, only as the square root of the said diameter: That we see this in the dust i



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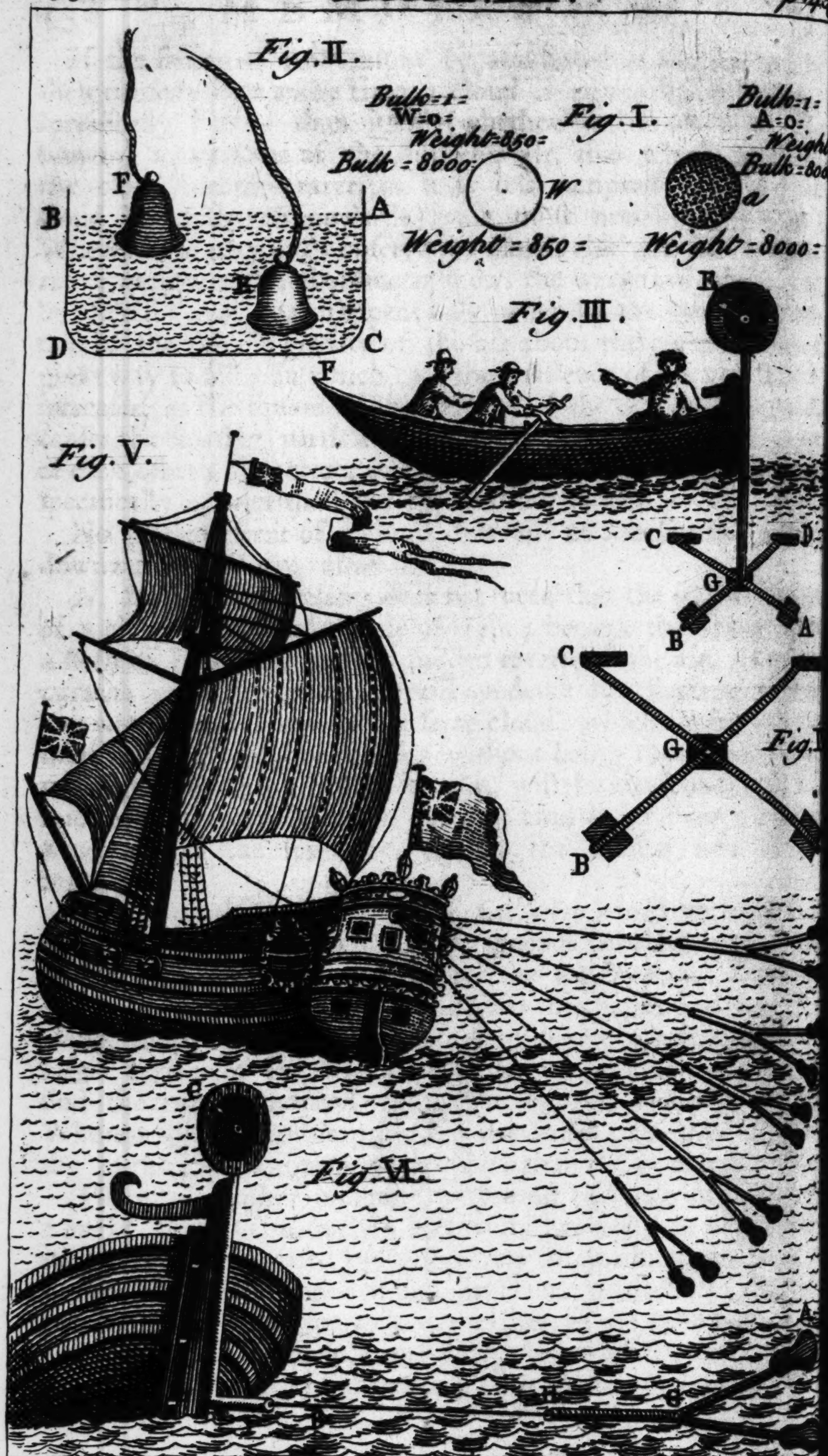

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in ſummer, and in menſtruum's that ſuſtain diſſolved metals, which are ſpecifically heavier than the ſaid menſtruum's.

But this will not explain the phenomenon; becauſe tho' the increaſe of ſurface (the weight remaining the ſame) will in a great meaſure hinder (or rather retard) the deſcent of ſmall bodies moving in the air, by reaſon of its great reſiſtence to ſo large a ſurface; it will for the ſame reaſon likewiſe hinder the aſcent: For, the riſe of duſt is owing to the motion of animals feet in it, or to the wind: Whereas vapours ariſe in calm weather, as well as windy; nor do they, like the duſt, always fall to the ground when the wind ceaſes to blow.

The third opinion, and which is moſt commonly received, is, that by the action of the ſun on the water, ſmall particles of water are formed into hollow ſpherules, filled with an *aura*, or finer air highly rarified, ſo as to become ſpecifically lighter than common air; and conſequently, that they muſt riſe in it by hydroſtatical laws: As for inſtance, if a particle of water, as it becomes a hollow ſphere, be only increaſed ten times in diameter, its bulk will be increaſed a thouſand times; it therefore, will then be ſpecifically lighter than common water, whoſe ſpecific gravity is to that of air, as 850 to 1; then if the denſity of the *aura*, or ſpirit within the little ſhell, be ſuppoſed 9 times leſs than that of air, or as 50 to 850, that ſpecific gravity of the ſhell and its contents, will be to that of air, as 900 to 1000; therefore, ſuch an aqueous bubble muſt riſe, till it come to an *æquilibrium* in air, whoſe denſity is to that in which it began to riſe, as 850 to 945 nearly. But it appears by experiments, that air rarified by a heat which makes a retort red hot, is only increaſed in bulk, or dilated three times; by the heat of boiling water, only $\frac{10}{14}$, or near $\frac{2}{3}$; and by the heat of the human body (ſuch as will raiſe vapours plentifully) only $\frac{10}{39}$, or about $\frac{1}{4}$.

Dr. *Deſaguliers* acknowledges, that his objection may be anſwered, by ſuppoſing the ſpherule of water to be more increas'd in diameter, as for inſtance, 20 times; becauſe then if it be filled with air, only $\frac{1}{4}$ rarer than common air, it will be ſpecifically lighter, and capable of riſing to a conſiderable height.

To give this ſolution all its force, let us expreſs it in numbers: Let A and W (Fig. 1. Plate XII.) repreſent a particle of air, and one of water of equal bulk; then will the weight of A be to that of W, as 1 to 850; their bulks being equal. If the particle of water be blown up into a bubble *w*, of 20 times its diameter; then will its bulk be to its weight as 8000 to

850;

850; whilst a sphere of air *a* of the same bigness has its weight as well as bulk equal to 8000: Now if an *aura* $\frac{3}{4}$ rarer than common air be supposed within the watery bubble to keep it blown; it will be the same, as if $\frac{3}{4}$ of the air of *a* were carried into *w*; and then the weight of *w* would be increased by the number 6000: So that the shell of water being in bulk 8000, would be in weight $850 + 6000 = 6850$, whilst an equal bulk of air weighed 8000; and consequently, the watery bubble would rise till it came to an air, whose density is to that of air next the surface of the exhaling water, as 6850 to 8000.

This is the strongest way of stating the hypothesis: But to support it, the following queries must be answered.

Q. 1. How comes the *aura*, or air in the bubbles, to be specifically lighter than the air without them; since the sun's rays, which act upon the water, are equally dense all over its surface?

Q. 2. If it could be possible for a rarer air to be separated from the denser circumambient air, to blow up the bubbles (as bubbles of soap water are blown up by warm air from the lungs; whilst the circumambient air is colder and denser) what would hinder that cold air by its greater pressure, from reducing the bubbles to a less bulk, and greater specific gravity than the air; especially since cold can be communicated thro' such thin shells; and the tenacity of common water is very small, when compared with that of soaped water, whose bubbles (notwithstanding that tenacity) are soon destroyed by the pressure of the external air, as the air within them cools?

Q. 3. If we should grant all the rest of the supposition; yet this difficulty will remain: If clouds are made up of hollow shells of water, filled with air, why do not those clouds always expand, when the ambient air is rarified, and presses less than it did before; and likewise suffer a condensation, as the ambient air is condensed by the accumulation of the superior air?

If this condensation and rarifaction should happen to the clouds, they would always continue at the same height, contrary to observation; and we should never have any rain.

From all this it follows, that the condensation and rarifaction of the vapours, which form clouds, must depend upon another principle than the condensation, and rarifaction of the air: And that there is such a principle, the Dr. endeavours to shew.

Lemma. The particles of all fluids have a repellent force.

Fluids

Fluids are elaſtic or unelaſtic: The elaſtic fluids have their denſity proportionable to their compreſſion, and Sir *Iſaac Newton* has demonſtrated *Princip. lib. 2. Sect. 5.* that they conſiſt of parts that repel each other from their reſpective centers: Unelaſtic fluids, like mercury, water, and other liquors, are by experiments found to be incompreſſible. For, water in the *Florentine experiment* could not by any force be compreſſed into leſs room; but ouzed like dew thro' the pores of the hollow golden ball, in which it was confined, when a force was applied to preſs the ball out of its ſpherical, into a leſs capacious figure: Now, this property of water, and other liquors, muſt be entirely owing to the centrifugal force of its parts, and not its want of vacuity; ſince ſalts may be imbibed by water without increaſing its bulk, as appears by the increaſe of its ſpecific gravity: So metals, which (ſingly) have a certain ſpecific gravity, beyond which they cannot be condenſed, will yet receive each other into their interſtices; ſo as to make a compound, ſpecifically heavier than the heavieſt of them; as is experienced in the commixture of copper and tin.

Scholium. By increaſing the repellent force of the particles, an unelaſtic, or incompreſſible, fluid may become elaſtic; or a ſolid (at leaſt a great part of it) may be changed into an elaſtic fluid; and *vice verſâ*, by diminiſhing the repellent force, an elaſtic fluid may be reduced to an unelaſtic one, or to a ſolid. That the particles of quickſilver, water, and other liquors, are likewiſe endued with an attractive force, is evident from thoſe ſubſtances running into drops in an exhauſted receiver, as well as in the air; and likewiſe from their adhering to other bodies. The attraction and repulſion exert their forces differently: The attraction does only act upon the particles, which are in contact, or very near it: In which caſe it overcomes the repulſion ſo far, as to render that fluid unelaſtic, which otherwiſe would be elaſtic; but it does not entirely deſtroy the repulſion of the parts of the fluid; becauſe it is on account of that repulſion, that the fluid is then incompreſſible. When by heat, or fermentation (or any other cauſe, if there be any) the particles are ſeparated from their contact, the repulſion grows ſtronger, and the particles exert that force at great diſtances: So that the ſame body ſhall be expanded into a very large ſpace by becoming fluid; and may ſometimes take up more than a million of times more room than it did in a ſolid, or incompreſſible fluid. *Vide* the *Queries* at the end of Sir *Iſaac Newton*'s *optics*. Thus is water, by boiling, and by leſs degrees of heat, changed into an

elastic vapour, rare enough to rise in air; and thus are oils and quicksilver in distillation made to rise in a very rare medium, such as remains in the red hot retort; and sulphureous steam will rise even in an exhausted receiver; as the matter of the *aurora borealis* does in the thinner part of our atmosphere. If *aqua fortis* be poured on quicksilver, a reddish fume will rise much lighter than common air; so likewise will fumes rise from filings of metals, from vegetables when they ferment by putrifaction; and (as Mr. *Hales* has shewn) several solid substances by distilling, as well as by fermentation, will generate permanent air.

That heat will add elasticity to fluids is evident from numberless experiments; especially from distilling and chemistry: But what is needful to consider here is only, that it acts more powerfully on water than on common air: For, the same heat which rarefies air only $\frac{1}{8}$, will rarefy water very near 14000 times, changing it into steam, or vapour, as it boils it: And in winter that small degree of heat, which in respect to our bodies appears cold, will raise a steam, or vapour, from water, at the same time that it condenses air.

By a great many observations, made by Mr. *Henry Beighton* and Dr. *Desaguliers*, upon the engine to raise water by fire, according to Mr. *Newcomen*'s improvement of it; they found that the water in boiling is expanded 14000 times to generate a steam as strong (*i. e.* as elastic) as common air; which, therefore, must be 16 times and $\frac{1}{2}$ specifically lighter: And that this steam is not formed of the air, extricated out of the water, is plain; because it is condensed again into water by a jet of cold water spouting in it; and the little quantity of air that comes out of the injected water must be discharged at every stroke, otherwise the engine will not work well. There is likewise another experiment to confirm this, as follows.

A B C D (Fig. 2.) is a pretty large vessel of water, which must be set upon the fire to boil: In this vessel must be suspended the glass-bell E, made heavy enough to sink in water; but put in in such a manner, as when upright to be filled with water, without any bubbles of air at its crown on the inside; the crown being all under water. As the water boils the bell will by degrees be emptied of its water, being pressed down by the steam which rises above the water in the bell: But as that steam has the appearance of air; in order to know whether it be air or not, take the vessel off the fire, and draw up the bell by a string, fastened to its knob at top, till only the mouth remain

under

under water: Then as the ſteam condenſes by the cold air on the outſide of the bell, the water will riſe up into the bell at F quite to the top, without any bubble above it; which ſhews that the ſteam which kept out the water was not air.

N. B. This experiment ſucceeds beſt, when the water has been firſt exhauſted of its air, by boiling, and by the air-pump.

We know by ſeveral experiments, made on the fire-engine (in Captain *Savery*'s way, where the ſteam is made to preſs immediately on the water) that ſteam will drive away air, and that in proportion to its height; tho' in the open air it floats and riſes in it like ſmoke.

Now, if the particles, turned into ſteam, or vapour, repel each other ſtrongly, and repel air more than they do each other; aggregates of ſuch particles, made up of vapour, and vacuity, may riſe in air of different denſities, according to their own denſity, dependent on their degree of heat, without having recourſe to imaginary bubbles, formed in a manner that is only ſuppoſed, and not proved, as has been already ſhewn.

Dr. *Deſaguliers* owns, indeed, that if the watery particles had no repellent force, they muſt precipitate in the ſame manner that duſt will do, after it has been raiſed up: But we have too many obſervations and experiments to leave any doubt of the exiſtence of the repellent force above-mentioned: Nor can it be ſhewn by any experiment, how big the *moleculæ* muſt be, that exclude air from their interſtices; and whether theſe *moleculæ* vary in proportion to the degree of heat, by an increaſe of repellent force in each watery particle, or by a farther diviſion of the particles into other particles ſtill leſs: But in general, we may reaſonably affirm, that the rarity of the vapour is proportionable to the degree of its heat, as it happens in other fluids (vide *Phil. Tranſ.* N° 270.) and that tho' the different degrees of the air's rarefaction are alſo proportionable to the heat, the ſame degree of heat rarefies vapour much more than air.

Now to ſhew, that what has been ſaid will account for the riſe of vapours, and formation of clouds, we muſt only conſider whether that degree of heat, which is known to rarefy water 14000 times, being compared with ſeveral of thoſe degrees of heat in ſummer, autumn and winter, which are capable of raiſing exhalations from water, or ice; the rarity of the vapours (eſtimated by the degree of heat) will appear to be ſuch that the vapour will riſe high enough in winter, and not too high in ſummer, to agree with the known phenomena.

That the effects are adequate to the causes in this case, the Dr. thinks he can make out in the following manner.

The heat of boiling water, according to Sir *Isaac Newton's* table (Phil. Transf. N° 270.) is 34; the mean heat of summer 5; the mean heat of spring or autumn 3; and the least degree of heat (or the mean heat of winter) by which vapours rise is 2: The rarity of vapour proportionable to these 4 degrees of heat, is 14000, 2058, 1235, and 823. The rarity of air is in summer 900, in spring or autumn 850, and in winter 800; the density of water, compared with the above mentioned densities being inversely as 1 to the abovesaid 4 numbers. The heights above the earth, to which the vapours will rise, and at which they will be in *equilibrio*, in an air of the same density with themselves, will vary according to the rarity of the vapour depending on the heat of the season. For, the vapour which is raised by the heat of winter, expressed by the number 2, when the rarity of the air is 800, will rise to, and settle at a height of about the sixth part of a mile; when the barometer is above 30 inches high: But if the heat be greater then, the vapours will rise higher; and pretty much higher, if the sun shine (tho' in frosty weather) the barometer being then very high: If the barometer fall, and thereby bring the place of *æquilibrium* (for vapours, raised by the heat 2) nearer the earth; then likewise will the heat be increased, the vapour more rarefied; and consequently, the new place of *æquilibrium* sufficiently high. It is to be observed, that in winter, when the heat is only equal to 2, the air is densest close to the earth, that has not any heat sufficient to rarefy it near the ground, as happens in warm weather; the vapour therefore, will gradually rise in an air, whose density decreases continually from the earth upwards; neither will the vapour be hindered of its full rise, by any condensation from a greater cold of the ambient air; the air being then as cold next to the ground, where the vapour begins to rise, as it is at any height from the earth.

The vapour, which is raised by the heat of spring, or autumn, expressed by number 3, will rise to the height of 3 miles and ½; when the barometer is at 30, and the rarity of the air is 850: But then as the air is hotter nearer the ground than at the height of half a mile, or a mile, the vapour will condense as it rises; and as the air, when the earth is heated, is rarer near the ground than at some height therefrom; the place of *æquilibrium* for vapour will, upon these two accounts, be brought much lower than otherwise it would be; as for instance, to

to the height of about a mile, which will agree with phenomena

In ſummer the two cauſes above-mentioned increaſing; the vapour raiſed by the heat 5 (whoſe place of *æquilibrium* would be 5 miles and ½ high; if the vapour, after it began to riſe, were not condenſed by cooling, and the air were denſeſt cloſe to the earth) will ſettle at the height of about 1 mile and ½ or 2 miles; which is likewiſe agreeable to phenomena.

Laſtly, as the denſity and rarity of the vapour is chiefly owing to its degree of heat, and in a ſmall meaſure to the increaſed, or diminiſhed preſſure of the ambient air, when it is not confined; and the denſity and rarity of the air is chiefly owing to the increaſed, or diminiſhed preſſure, by the accumulation, or exhauſtion of the ſuperior air; whilſt heat and cold alter its denſity in a much leſs proportion; the clouds formed of the abovementioned vapours, inſtead of conforming themſelves to the altered denſity of the ambient air, will riſe, when it is condenſed, and ſink when it is rarefied; and likewiſe riſe or ſink (when the preſſure of the air is not altered, and its denſity very little changed) by their own dilatation, owing to heat or cold; as may be often obſerved, by ſeeing them change their height conſiderably, whilſt the barometer continues exactly at the ſame degree, and the liquor of the thermometer riſes or falls very little, and ſometimes not at all.

As to the manner how clouds are changed into rain; the Dr. has hinted it in the beginning of this *Tranſaction*: But for farther ſatisfaction he refers the reader to Dr. *Halley*'s account of it *Phil. Tranſ.* N° 183. in which the former entirely acquieſces, having always found it agreeable to the phenomena.

Since the Dr. has only mentioned at what heights from the ſurface of the earth, vapours of differentt denſities will come to an *æquilibrium*, without giving a reaſon for ſettling the place of *æquilibrium* at thoſe heights, he here gives the method by which they are to be found, *viz.* as the vapours will ſettle and riſe, where the air is of the ſame denſity with themſelves; it is only required to find the denſity of the air at any diſtance from the earth, at ſeveral heights of the barometer, which may be deduced from Dr. *Halley*'s two tables *Phil. Tranſ.* N° 386 (the firſt ſhewing the altitude to given heights of the mercury; and the ſecond the heights of the mercury at given altitudes) and knowing the degree of heat by the thermometer; becauſe the denſity of the vapours depends upon the degree of heat of the ſeaſon; provided that proper allowances be made for the great

rare-

rarefaction of the air near the earth in hot and dry weather and the condenſation of the vapours in their riſe; by reaſon of the air being colder at a little height above the earth than juſt at its ſurface.

Some Obſervations on the Peak *in* Derbyſhire; *by Mr.* John Martyn. Phil. Tranſ. N° 407. p. 22.

THE *peak* in *Derbyſhire* is famous for 7 places, which our anceſtors have deemed wonders: 1. *Chatſworth* (a magnificent ſeat of the Duke of *Devonſhire*) 2. *Mam-tor*; 3. *Elden-hole*; 4. The *ebbing and flowing well*; 5. *Buxton-well*; 6. *Peak's hole*, and 7. *Pool's hole*.

The firſt being a work not of nature but art does not come within the deſign of this account.

Mam-tor is a huge precipice facing the eaſt, or ſouth-eaſt; which is ſaid to be perpetually ſhivering, and throwing down large ſtones on a ſmaller mountain below it; and that nevertheleſs, neither does the one increaſe nor the other decreaſe in bigneſs. This mountain is chiefly compoſed of a ſort of ſlate-ſtone (called in that country *black ſhale*) and large ſtone. The nature of the *black ſhale* is ſuch, that tho' it be very hard before it is expoſed to the air; yet it afterwards very eaſily crumbles to duſt: Thus upon any ſtorm, or melting of ſnow, this *ſhale* is conſiderably waſted; and as the large ſtones are gradually diſengaged, they muſt neceſſarily fall down: That it is only at theſe times that the mountain waſtes, is affirmed by the moſt intelligent of the neighbouring inhabitants; and that this decay is not continual, Mr. *Martyn* himſelf could affirm; having not only taken a cloſe ſurvey of it, but likewiſe climbed up the very precipice, without obſerving any other ſhivering in the mountain, than what the treading of his own feet in the looſe crumbled earth occaſioned. That the mountain does not decreaſe in the mean time, is a tale too frivolous to need any conſideration.

Elden-hole is a huge perpendicular chaſm: Its depth is not known. Mr. *Cotton* tells us, that he ſounded 884 yards; and yet the plummet drew, but he might eaſily be deceived, unleſs his plummet were of a very great weight: For, otherwiſe Mr. *Martyn* imagines the weight of a rope of that length would be ſo great, as to make the landing of the plummet ſcarce perceivable: Be that as it will, its depth, to be ſure, is very conſiderable; and ſince we have no where in *England* ſo good an opportunity of ſearching the bowels of the earth

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to so great a depth; he is surpris'd no curious person has ever had the courage to venture down. It is said, indeed, that one was hir'd to be let down with a rope about his middle 200 yards; and that he was drawn up again, out of his senses, and died a few days after; and no wonder: For, the poor man, reflecting in that dismal place on the danger he had put himself into for the sake of a little money, might probably be frighted out of his senses; or indeed, the very fatigue itself might put him into that condition: But Mr. *Martyn* conceives, that if any intelligent and prudent person was to be let down in a proper machine, he would not be much in danger, and his fatigue would be very inconsiderable.

The ebbing and flowing well is far from being regular, as some have pretended: It is very seldom seen by the neighbours themselves; and as for Mr. *Martyn* he waited a good while at it to no purpose.

Buxton well has been esteem'd a wonder on account of two springs; one warm and the other cold, rising near each other: But the wonder is now lost, both being blended together. The spring, which is now us'd for bathing, appears to be 32 degrees and a half of one of Mr. *Hawksbee*'s thermometers warmer than the common spring water there. The spring-water kept the spirit of wine at 41; and the bath-water rais'd it to eight and a half.

Peak's *hole* and *Pool*'s *hole* are two remarkable horizonal openings under mountains; the one near *Castleton*, and the other just by *Buxton*. They seem to Mr. *Martyn* to have ow'd their original to the springs, which have their current thro' them. It is easy to imagine, that when the water had forced its way thro' the horizontal fissures of the *strata*, and carried the loose earth away with it, the loose stones must of course fall down; and that where the *strata* had few or no fissures, they remain'd entire; and so form'd those very irregular arches, which are so much wondered at in those places. This seems more probable to him, than what others have hitherto propos'd. The three rivers (as they are commonly call'd) in *Peak*'s *hole* are only some parts of the cave deeper than the rest; and receiving all their water from the spring which comes from the farther end of the cave. The water, which passes thro' *Pool*'s *hole*, is impregnated with particles of lime-stone; and so has incrusted almost the whole cave in such a manner, that it appears like one solid rock.

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The lead mines in *Derbyſhire* are very various with regard to their courſes: One, into which Mr. *Martyn* went down, had two branches; one running to the N. E. the other to the N. W. and as he was informed, one of the beſt they ever diſcover'd ran due N. their breadth and depth are full as irregular: The bodies thro' which they dig to come at the vein, are generally lime-ſtone and black ſhale; but it is uncertain which of the two is uppermoſt. Of two mines into which he went down; in one they had dug firſt thro' 20 yards of lime-ſtone; then thro' one yard of black ſhale: In the other, firſt thro' 42 yards of ſhale; and then thro' 28 yards of lime-ſtone. The ſubſtances, which they find mixt with the ore, are

1. *Chert:* This is a kind of flint, which Dr. *Woodward*, in his *Method of foſſils*, p. 21. ſays is called ſo, when it is found in thin *ſtrata:* But in the *peak* the *ſtrata* of *Chert* are often four yards thick, or thicker. They are found in lime-ſtone, and not always diſpos'd in *ſtrata.* Thoſe Mr. *Martyn* took notice of were generally either black, or of ſuch a colour, as the inſpiſſated juice of the buckthorn berries, which painters call *ſap-green:* Whence they are denominated green *cherts* and black *cherts.*

2. *Spar:* This is compos'd of cryſtal mixt with other bodies. Thoſe they call ſugar-ſpars are ſuch, whoſe cryſtallizations are very ſmall, and ſo upon crumbling to pieces reſemble powder'd ſugar. Mr. *Martyn* had two ſorts of theſe, white and blue. *Dog-tooth ſpar* is a white pointed ſpar, in form and colour ſomewhat reſembling teeth.

3. *Cauk.* This Dr. *Woodward ibid. p.* 18, ſays, is a kind of coarſe talcy ſpar: But in that ſubſtance Mr. *Martyn* met with in this country under the name of *cauk*; he could not diſcover any flexibility, or elaſticity, which that learned writer has ſet down as characteriſtics of talc, and talcy bodies. It ſeems to Mr. *Martyn*, *Catalogue of foſſils*, Vol. I. part 1. p. 57. to be nothing but ſpar, incorporated with a coarſe earthy matter. When this cauk is mix'd with pellucid cryſtallizations of ſpar, it is call'd *baſtard cauk.*

There are ſeveral other bodies mixt with the mines with lead-ore: But as they do not occur in thoſe mines he examined, he omits mentioning them.

As to the working the lead-ore; when it is landed, or brought up from the mine, it is broken to pieces, that the ſpar, cauk, or other bodies, which adhere to it, may be the

more easily separated. It is then thrown into a large sieve, and wash'd; and so farther purified from extraneous bodies. After this, it is carried to the furnace in order to be smelted. The furnace Mr. *Martyn* saw near *Worksworth* was very rude and simple, consisting only of some large rough stones, placed in such a manner, as to form a square cavity, into which the ore and coals are thrown *stratum super stratum*; two large bellows continually blowing the fire, and mov'd alternately by water. Mr. *Martyn* observ'd no other fuel, us'd on this occasion, but dried sticks, which they call *white coal*. Mr. *Ray*, *Collection of English Words*, *Edit*. 2. *p*. 174. informs us, that they use both white and black coal, or charcoal in *Cardiganshire*; probably, because that ore is harder to flux, the charcoal making a more vehement fire. They throw in some spar along with the ore, which is suppos'd by imbibing the sulphur to make it flux more easily: They also frequently throw in some *cowke* (or cinders of pit-coal) because they think it attracts the dross, and so makes an easier separation of it from the lead. When the ore is melted, it runs out at an opening in the bottom part of the front of the furnace, thro' a small channel made for that purpose, into a cylindrical vessel, out of which it is laded into the mould. The dross of the ore upon smelting, is call'd *slag*, which is afterwards smelted again with *cowke* only; and the lead, obtain'd from it, is call'd *slag-lead*. Their way of making *red-lead* is the same with Mr. *Ray*'s account, *ibid*. *Collect*. p. 200; only they use three parts of lead, and one of slag-lead; and think that the red-lead made in this manner is better than if made without slag-lead.

In his way to the *Peak* in *Derbyshire*, Mr. *Martyn* took notice of the following plants, which he had not observ'd to be common in other parts of *England*, and are not taken notice of by the Bishop of *London*, in his edition of *Camden*.

Stachys Fuschii I. B. in the road to *Grantham*, a little beyond *Colesworth*.

Scrophularia Scorodoniæ, *folio mor*. At *Wollerton* under the garden-wall.

This does not owe its origin in this place to seeds, scatter'd out of the garden; as Mr. *Martyn* is convinced, by perusing a manuscript catalogue of the plants, cultivated in that garden, in which there is no mention made of this plant.

The *Lychnis*, which grows on *Nottingham Castle*, is the *Lychnis sylvestris alba 9 Clusii*; and not the same with Mr. *Ray's lychnis major noctiflora Dubrensis perennis*, as he suspected.

Festuca humilior panicula brevi heteromalla; gramen paniculatum, bromoides, minus, paniculis aristatis, unam partem spectantibus Raii syn. On *Sherwood* forest.

Salix folio laureo, seu lato glabro odorato Phyt. Brit. Common about *Wingerworth*.

Ladanum arvenose, flore amplo luteo, labro purpureo. Lamium cannabinum, flore amplo luteo, labio purpureo Raii Syn. In the corn in several places.

Filix mas non ramosa, pinnulis angustis, raris, profunde dentatis Ger. emac. Common about *Wingerworth*.

The more rare plants he observ'd in the *Peak* are, as follows.

Scariola sylvestris Anguillaræ. Lactuca Sylv. murorum flore luteo I. B. On old walls, and about the entrance into *Peak's hole*: It likewise grows in *Hertfordshire*. Mr. *Martyn* takes notice of it here, the rather because M. *Vaillant* has evidently mistaken its characters in his new distribution of the cichoraceous tribe in the *Memoirs of the Royal Academy of Sciences* for the year 1721. He there makes it a species of *lactuca*, from which it is very different on his own principles. According to his method the empalement of the *lactuca* is squamous, and the down of the seed sits upon a pedicle: But this species has a simple empalement, and a sessile down. These characters evidentlydistinguish it not only from *lactuca*, but from every *genus* in his method: And therefore Mr. *Martyn* constitutes a new *genus*; and as the name of *Scariola*, by which *Anguillara* has call'd it, has not hitherto been appropriated to any other *genus*, he appropriates it to this, and defines it as follows.

Scariola is a cichoraceous plant, with a simple empalement, a naked *placenta*, and seeds crown'd with a hairy sessile down.

Rosa sylv. alba cum aliquo rubore foliis hirsutis I. B. In several hedges about *Hathersedge*.

Empetrum montanum fructu nigro Tourn. Common on the mountains.

Oxycoccus, seu vaccinia palustria, I. B. On boggy places, but not very common.

Erica

Erica humilis cortice cinereo arbuti flore albo, H. R. Par. On the mountains near *Hatherſedge.*

Rubus Idæus ſpinoſus fructu rubro, I. B. In the hedges.

Geranium ſaxatile Ger. emac. About the entrance into *Peak*'s *hole.*

Cochlearia rotundifolia minima Merr. About the entrance into *Peak*'s *hole.*

Thalictrum minus Ger. In the ſame place.

Lachenoides ſaxatile, fuſcum, piloſum, variè diviſum. Coralina fuſca folioſa Doody Budd. hort. ſicc. On the rocks.

Lichenoides ſaxatile tinctorium foliis piloſis purpureis Dillenii. On the rocks.

Uſnea ſaxatilis, capillacea. Muſcus corallinus, ſaxatilis, fœniculaceus Raii Syn. On the rocks near *Darwent.*

Licopodium Sabinæ facie Fl. Jen. On the mountains near *Darwent.*

Selago foliis & facie abietis Fl. Jen. On the mountains near *Darwent.*

Bryum hypnoides capitulis, plurimis erectis lanuginoſum Dillenii. On the mountains.

Cardamine impatiens altera hurſutior Raii Syn. About the mouth of *Pool*'s *hole* in plenty.

A variety of Mr. *Ray*'s *viola montana lutea* with a blue and yellow flower.

The Longitude *of divers Places computed from Observations of the* Eclipses *of* Jupiter's Satellites; *by Mr.* Derham. Phil. Transf. N° 407. p. 34.

H.	M.	S.
Rome and *Lisbon.*		
1	24	46
1	25	34
1	26	34
1	29	0
1	26	44
1	26	54
1	28	11
Rome and *Paris.*		
0	39	48
0	40	50
0	36	16
0	38	56
0	40	17
Rome and *Ingolstad.*		
0	2	51
0	4	1
Rome and *Bologne.*		
0	3	45
	2	16
0	4	45
0	4	14

H.	M.	S.
Rome and *Kew.*		
0	45	47
Rome and *Wanstead.*		
0	49	10
Rome and *Upminster.*		
0	47	28
Rome and *Southwick Northampton*		
0	47	58
Urbino and *Lisbon.*		
1	28	57
Paris and *Lisbon.*		
0	45	46
0	45	44
Paris and *Bologne.*		
0	34	30
0	34	0
0	38	32

H.	M.	S.
Ingolstad and *Lisbon.*		
1	22	53
1	23	21
Ingolstad and St. *Quirico.*		
0	1	20
0	1	40
Ingolstad and *Bologne.*		
0	1	53
Ingolstad and *Paris.*		
0	36	23
0	36	0
Ingolstad and *Upminster.*		
0	46	10
St. *Quirico* and *Lisbon.*		
1	22	30
St. *Quirico* and *Paris.*		
0	37	40

H.	M.	S.
St. *Quirico* and *Upminst.*		
0	47	50
Florence and *Lisbon.*		
1	19	43
Florence and *Bologne.*		
0	0	31
Florence and *Upminster.*		
0	42	1
Upminst. and *Bologne.*		
0	43	43
Upminst. and *Lisbon.*		
0	37	42
Bologne and *Lisbon.*		
1	21	28
Bologne and *Albano.*		
0	3	43

Eclipses of Jupiter's Satellites *from* 1721 *to* 1729; *by S.* Bianchini *and others.* Phil. Transf. N° 407. p. 35.

Days of the month.		Time of observation.			Satellite eclips'd.		Place where observed.
		Anno 1721					
		H.	M.	S.			
April	3	15	4	32	Immerf.	1	At *Rome.*
June	21	8	46	0	Emerf.	1	*Rome*

Days

Days of the month.		Time of observation. H.	M.	S.	Satellite eclipsed.		Place where observ'd.
		Anno 1722					
June	9	13	20	0	Em.	1	*Rome*
——	18	9	36	30	Em.	1	*Albano*
July	11	9	49	10	Em.	1	*Rome*
——	27	8	7	30	Em.	1	*Rome*
August	19	8	26	20	Em.	1	*Rome*
		1723					
March	26	17	14	50	Im.	1	*Rome*
April	11	15	31	45	Im.	1	*Rome*
May	3	{ 15	48	51	Im.	1	*Rome*
——		{ 15	43	0			*Ingolstad*, by F. *Grammatici*
——	27	18	56	0	Im.	1	*Rome*
June	5	12	16	30	Im.	1	*Rome*
——	12	14	11	39	Im.	1	*Rome*
July	23	{ 9	11	40	Em.	1	*Rome*
		{ 7	46	0			*Lisbon*, by *Carbone*
——	30	11	7	20	Em.	1	*Rome*
August	8	7	32	0	Em.	1	{ *Ocricoli* in *Viâ Flaminiâ*.
——	15	9	35	0	Em.	1	{ *Assisi* in *Ombria*.
September	7	{ 9	50	45	Em.	1	*Urbino*
		{ 8	21	48			*Lisbon*
——	23	8	17	54	Em.	1	{ *Muceria* in *Ombria*
October	16	8	36	10	Em.	1	{ *Albano* in the *Viâ Appiâ*
		1724					
June	8	{ 14	3	28	Im.	1	*Carbone* at *Lisbon*
		{ 15	56	27			
——	23	13	42	50	Im.	1	*Rome*
——	30	{ 15	34	29	Im.	1	*Rome*
		{ 14	8	55			*Lisbon*
August	10	10	45	20	Em.	1	*Rome* doubt.
——	17	12	40	45	Em.	1	*Rome*

Days of the month.		Time of observation. H.	M.	S.	Satellite eclipsed.		Place where observ'd
				1724			
August	26	9	6	45	Em.	1	*Rome*
September	11	7	30	53	Em.	1	*Rome*
———	18	9	28	16	Em.	1	*Rome*
———	25	11	25	55	Em.	1	*Rome*
		9	59	21			*Lisbon*
October	11	9	53	8	Em.	1	*Albano*
———	14	9	31	0	Em.	3	From *Jupiter*'s limb
		11	7	0	Im.	3	into his shadow at
		11	9	30			*Albano*
———	27	8	16	0	Em.	1	*Albano*
November	12	5	33	10	Em.	1	*Rome*
———	19	8	25	5	Em.	1	*Rome*
———	30	6	14	0	Im.	1	*Peking* in *China* by F. [*Koegler*
December	5	6	42	25	Em.	1	*Rome*
				1725			
June	19	15	17	10	Im.	1	*Rome*
July	5	13	32	20	Im.	1	*Albano*
———	7	14	55	30	Im.	1	*Pekin*
———	21	11	45	22	Im.	1	*Rome*
		10	39	35			Mr. *Molyneux* near *London*
———	28	13	39	10	Im.	1	*Rome*
		12	12	26			*Lisbon*
November	15	9	53	50	Em.	1	*Rome*
		8	24	50			*Lisbon*
———	14	6	15	15	Em.	1	*Rome*
December	17	6	20	30	Em.	1	*Rome*
				1726			
July	17	13	28	46	Im.	1	*Rome*
		13	24	45			*Ingolstad*
		12	1	52			*Lisbon*
August	2	11	40	0	Im.	1	St *Quirico* in *Tusc.*
		11	41	20			*Ingolstad*
———	9	13	36	0	Im.	1	*Sienna* in *Tuscany*
		12	13	30			*Lisbon*
———	16	15	28	29	Im.	1	*Florence*
		14	8	46			*Lisbon*
		15	29	0			*Bologne*

Days

Days of the month.		Time of observation. H.	M.	S.	Satellite eclips'd		Place where observ'd
		Anno 1726.					
August	25	11	54	24	Im.	1	*Bianch.* at *Bologne*
		11	54	26			*Manfr.* at *Bologne*
		11	56	18			*Ingolstad*
		11	19	55			*Paris*
		10	32	57			*Lisbon*
September	26	8	41	0	Im.	1	St. *Quirico*
		8	39	20			*Ingolstad*
		8	3	20			*Paris*
October	1	16	7	45	Im.	1	St. *Quirico*
November	20	7	46	30	Em.	1	*Rome*
		6	20	19			*Lisbon*
———	27	9	39	25	Em.	1	*Rome*
December	6	6	0	16	Em.	1	*Rome*
		5	58	0			*Bologne*
		5	24	0			*Paris*
		1727					
March	8	6	42	50	Em.	1	*Rome*
August	5	15	18	27	Im.	1	*Rome*
		15	0	8	Im.	1	*Rome*
		14	21	12			*Paris*
September	6	12	0	0	Im.	1	*Rome*
		11	55	15			*Bologne*
		11	19	43			*Paris*
October	15	10	41	30	Im.	1	*Albano*
———	20	6	5	54	Im.	1	*Albano*
———	22	12	33	23	Im.	1	*Albano*
		1728					
January	15	13	13	46	Em.	1	*Rome*
February	16	9	46	56	Em.	1	*Rome*
March	26	8	32	7	Em.	1	*Rome*

Eclipses

Eclipses *of* Jupiter's Satellites *from* 1726 *to* 1728, *at the Observatory of* Bologne; *by* S. Manfredi. Phil. Trans. N° 407. p. 36.

Days of the month.		Time of observation.			Satel. eclipsed.		
		Anno 1726					
		H.	'	"			
August	16	15	29	0	Im.	1	Dubious.
——	25	11	54	24	Im.	1	Dubious
November	27	9	35	11	Em.	1	Dubious.
December	4	11	27	45	Em.	1	Dubious.
——	26	5	47	4	Im.	3	Dubious.
——		7	56	23			The 3d began to emer.
——		29	59	26	Em.	1	
——	31	6	18	54	Em.	2	Just begun.
		Anno 1727					
January	2	9	45	27	Im.	3	Dubious.
		11	53	38	Em.		
——	5	7	51	54	Em.	1	
——	7	8	54	12	Em.	2	
February	7	5	50	5	Im.	3	Dubious.
		7	52	54	Em.		
——	8	8	37	59	Em.	2	Air thick.
August	21	13	34	39	Im.	1	
September	6	11	55	17	Im.	1	
——	17	10	48	59	Im.	3	
		12	40	30	Em.		
October	13	16	5	45	Im.	1	
——	22	12	29	42	Im.	1	
——	23	8	55	34	Em.	3	
——	30	11	1	9	Im.	3	Dubious.
November	5	9	5	15	Im.	2	Dubious.
——	30	8	44	13	Em.	2	
		Anno 1728					
January	17	8	41	8	Em.	3	
February	16	9	43	11	Em.	1	
——	29	6	40	45	Im.		Dubious.
		8	50	40	Em.		

N. B. S. *Bianchini*'s observations were made with one of *Campani*'s telescopes, 23 *Roman* palms and ½ long; F. *Carbone*'s

bone's by another of the same make, length, and goodness. The observations at *Paris* were made by M. *Maraldi*. They were all put in this view by Dr. *Derham*, who supposes there is a mistake in the observations of *November* 30, 1724; that it was an emersion; not an immersion.

Eclipses *of* Jupiter's Satellites *at* Petersburgh *in* 1726, 1727, 1728; *by M.* De Lisle. Phil. Trans. N° 407. p. 37.

Days of the month.		Time of observation.			Satel. eclipsed.		Telescope.	
		Anno 1726						
		H.	′	″				
July	10	12	47	0	Im.	1	15 foot	A little doubtful.
August	9	14	51	30	Im.	1	15 & 22 f.	Doubtful near 15″.
	18	11	15	46	Im.	1	15	
		11	15	52	Im.		20 ½	
September	10	11	32	51	Im.	1	15	
		11	32	56	Im.		20 ½	
	22	16	13	20	Im.	2	15	
October	19	12	21	46	Em.	1	15	
	28	8	47	8	Em.	1	15	To some seconds.
December	6	7	11	18	Em.	1	20 ½	Somewhat doubtful.
		10	30	31	Em.	2	20 ½	Exact.
		10	30	38	Em.		15	Exact.
	29	7	15	36	Em.	1	20 ½	Exact.
		7	15	48	Em.		15	Exact.
		1727						
January	2	10	59	46	Im.	3	15	Air foggy.
		11	0	17	Im.		20 ½	
	7	10	9	56	Em.	2	20 ½	
		10	10	4	Em.		15	
February	1	7	17	15	Em.	2	20 ½	Exact.
August	5	11	52	23	Im.	3	22	To a few seconds.
	7	10	59	27	Im.	1	22	*Jupiter* was low.
	8	13	37	9	Im.	2	22	
	21	14	50	30	Im.	1	22	
	30	11	19	18	Im.	1	22	
September	2	10	43	57	Im.	2	22	
	6	13	11	24	Im.	1	22	Doubtful.
	9	13	21	35	Im.	2	22	
	10	9	34	30	Em.	3	22	
	15	9	36	32	Im.	1	22	
October	31	10	8	48	Im.	1	15	Doubtful.
December	2	8	46	30	Em.	1	22	
	12	14	6	0	Em.	3	22	

Days of the month.	Time of observation.			Sat. eclipsed.		Telescope.	
	Anno 1728						
	H.	′	″				
January 8	12	14	44	Em.	2	15 foot	To some seconds.
	12	33	34	Em.	1	13	Somewhat doubtful.
——10	5	58	7	Em.	3	22	To some seconds.
	7	0	12	Em.	1	22	The wind incommoded
——17	7	56	31	Im.	3	13	
	8	53	4	Em.	1	22	Exact.
	9	55	14	Em.	3	22	
February 16	10	59	26	Em.	1	22	To some seconds.
——18	5	28	20	Em.	1	15	The day not closed.
——27	6	40	5	Em.	2	22	
——29	8	0	22	Im.	3	22	The Sat. appeared and disappeared a different times.
March 10	11	18	19	Em.	1	13 and 15	
April 12	8	16	12	Im.	3	15	
	10	30	40	Em.	3	15	*Jupiter* was low.

Queries *concerning the Cause of cohesion of the Parts o* Matter; *by M.* Triewald, Phil. Transf. N° 408. p. 39.

Query 1. DOES not the strong cohesion of two balls o lead prove the doctrine of attraction, worthy it great author, Sir *Isaac Newton*; and that there is an universa attraction between the parts of matter in nature (tho' some a such small distances as to escape our observations) since we cannot make their parts touch one another close enough; so as to come within their sphere of activity? Which M. *Triewald* presumes to be the reason, why he never could make balls of any other metals to cohere: Nor does he believe, that the parts o any other metal can come to such a close contact, except by fusion; as the particles of lead may, by being so many degrees softer than those of any other metal.

Q. 2. M. *Triewald* has often found the touching surfaces o such leaden balls, as near as he could measure, much alike; yet the force of cohesion very different: Nay, he has found the touching surfaces very small; yet sometimes 114 to 126 *lb.* weight has not been sufficient to separate them; when at other times a far less weight (tho' the measure of touching surfaces fa exceeded those mentioned) was more than sufficient to cause their separation. Does it not prove that the cohesion is strongest, according to the closeness of the contact, but not as the touching

ing ſurfaces? For which reaſon he always found the coheſion ſtrongeſt, when he gave a little twiſt in joining them; ſince by this means the particles muſt come cloſer together than by ſqueezing the balls barely on each other, tho' it were done with a far greater force than he could apply with his bare hands. And ſince the force, twiſt and touching ſurfaces, can never be alike, and menſurable when joined by hand, he thinks it will be very difficult, if not impoſſible, to aſcertain the forces of this coheſion, which is incredible, and far exceeds magnetical attractions.

That the preſſure of the atmoſphere contributes little, and next to nothing in this coheſion, M. *Triewald* had fully proved, and experienced the winter before, in preſence of a great and noble aſſembly. The coheſion of two leaden balls, which 126 *lb.* could not ſeparate, proved as ſtrong *in vacuo*, as in the open air.

Q. 3. Does not this experiment fairly account for the coheſion of the parts of matter; and that this firm coheſion cannot be derived from any glue, or cement, any imaginary hooks and *funiculus*, nor from the gravity of the æther: But that the particles of all fluid and ſolid bodies attract one another by a certain force (whatever be the cauſe) which acts moſt intenſely, the nearer they touch one another.

M. *Triewald* is confirmed in this opinion by an experiment he made in ſummer at *Dannemora*, one of the moſt conſiderable iron mines in *Sweden*; and where M. *Triewald* erected the firſt and largeſt fire-engine for drawing water and ore; the cylinder being two lines more than 36 inches in diameter.

The *Dahlkarlians* have, time out of mind, practiſed the ſaid experiment, when they have occaſion to remove any unwieldy ſtones of the hardeſt rocks; and ſo big, as not to be moved entire by any ſtrength they could apply. They practiſe the following means, not only to cleave and ſplit them in as many parts and pieces, as they pleaſe, but likewiſe to obtain ſtones with one or more ſmooth ſides fit for uſe in building: They take tallow, greaſe, train-oil, or any other fat ſubſtances, and draw lines on ſuch large ſtones, according as they would have them ſplit; then they lay either charcoal, or wood at top, and round the ſides of the ſtone, ſo that it is all over covered, and then kindle the fuel; which when burnt, they find the ſtone divided, according to the lines they have drawn thereon with ſome of the before-mentioned fat ſubſtances, which ſeldom or never fails.

May not one account for this odd phenomenon thus? That as the action of heat and fire expands the parts of all hard and solid bodies, and even metals themselves; so when the action of the fire about the stone has made the particles of the same recede farther from one another, than when in their natural state, the oily substances insinuate themselves more and more between the particles of the stone; by which means when the stone cools again, and shrinks, they seem to prevent these particles from coming as close, and within their sphere of activity, as the remaining particles may, where no such foreign matter has been applied; by which means also they cannot attract one another so strongly as the rest; and must, therefore, remain separated.

Fat and oily substances seem to be most fit for this purpose; since they are endued with a repelling force.

Notwithstanding so many phenomena in nature prove a tendency, and a strong mutual attraction of the parts of matter, whatever be the cause; yet most learned men of several nations, would rather charge such manifest qualities, and operations of nature with the nick-name of occult qualities, than give the honour to the *Great Discoverer* of those manifest qualities and principles of motion. However, he is confident, that as nature is very uniform, and agreeable to herself, she will evince the truth of her operations.

Of the Nature and Virtues of the Holt-waters; *by Mr.* Lewis. Phil. Transf. N° 408. p. 43.

EXperience has prov'd the *Holt-waters* to be of admirable efficacy in scorbutic and scrophulous cases; wherein they have performed such wonders, that a short account, which was published of their cures in that kind, upwards of five years before, was looked upon by some, rather as a romantic tale, than a true narrative of real facts.

They are of an attenuating, astringent, and drying nature: And by these qualities, Mr. *Lewis* imagines, they perform their cures. The first is the known quality of all water, namely to dilute the blood, and thin the juices; and thereby fit them to pass the fine strainers, and be carried out of the body by their proper drains: In the second consists the great excellence of *Holt-water*, which, by its notable astringency, braces the solids, stimulates the fibres, and quickens their contractile power; and thereby enables them to shake off, protrude, and squeeze out such feculencies, as may adhere to, clog, and stuff them

them up: And this quality, it is probable, they derive from the allum, and iron, which are suppoſed to impregnate them. The ingredients, which give them their drying, abſorbing, and healing quality, are the ſulphur and ocre; by which they imbibe the peccant humours, and ſheath the ſharp ſalts, that lance and tear the finer glands, and cauſe blotches, and ulcerations. As they attenuate and aſtringe; they are a noble diuretic, removing obſtructions from the kidneys, and cauſing the renal glands to perform their proper ſecretions; and at the ſame time diſſolving the groſſer ſalts, and fitting them to be carried off thro' the urinary paſſages.

Theſe waters have been found of very great benefit in ſeveral other ailments, beſides the ſcurvy and *evil*.

The ſequel of the Account of a New Machine, *called the* Marine Surveyor, *contrived for the Menſuration of the Way of a Ship in the Sea; more correctly than by the* Log; *by Mr.* Henry De Saumarez. Phil. Tranſ. N° 408. p. 47.

F (Fig. 3. Plate XII.) repreſents Mr. *De Saumarez*'s boat on the *Canal* in St. *James's Park*; thro' the rudder of which a ſmall ſpindle paſſes (in an iron pipe) of which H G is the length. To the point G are faſtened the four iron fins, or flyers, A, B, C and D, in the form of a ſquare, the bars D B and A C, to which they are fixed, lying in an horizontal poſition. Theſe flyers are contrived in ſuch a manner, as to have full play in any motion of the boat. To the point H, which is the upper part of the pipe and ſpindle, is fixed the dial E: Now the boat being put in motion, the flyers move accordingly, which proportionally affecting the ſpindle, the motion is thereby communicated to the dial, which may be fitted to ſtrike the miles, or leagues, that the veſſel runs.

But to deſcribe the firſt movement of this machine more exactly; Fig. 4. repreſents it unfix'd: The croſs, or bars, D B, and A C, as is ſaid before, lie flat, or in a horizontal poſition; the arbor or ſpindle, which is perpendicular thereto, ſcrews into the point G, and paſſes thro' an iron pipe to the dial, in the manner aforeſaid. The flyers A, B, C, and D, being fitted to move in any motion of the boat, the bars are accordingly affected. This inſtrument is contrived in ſuch a manner, that two of the flyers on one ſide ſhall always reſiſt the water in the motion of the veſſel; whilſt the other two give way in their turning. The reſiſting flyers in this Fig. are A and B; and D and

and C will be the ſame when they come into their poſition: For, they reſiſt and give way alternately, as long as the motion continues, which is always circular; and ſo truly does it revolve, that be the motion ſwift or ſlow, in any meaſured diſtance, the number of revolutions will be equal.

This is the machine Mr. *De Saumarez* firſt tried on the *Canal* in St. *James's Park*, which he choſe the rather to do, in regard he found it to anſwer very well in all his experiments: And he is ſtill of opinion, that it would be an uſeful inſtrument to determine the ſtrength of the tides on our ſea-coaſt, which if marked in our charts, might prove advantageous to our commerce: But conſidering, that tho' this projection might be ſerviceable in barges, pleaſure-boats, or other veſſels, in fair and moderate gales of wind; yet it might prove uſeleſs in boiſterous and ſtormy weather, and in long voyages, when it might be choak'd with weeds; he, therefore, fix'd to his other invention the fork, which is contrived in ſuch a manner, that he will even ſtill be ſo bold as to affirm, it ſhall determine the ſhip's way in a ſtorm, or when ſhe is ſcudding before the wind, when the log is incapable of it. As the *Canal* would not allow him to try, with any certainty, his iron forks there, he was obliged to have ſome made of lighter materials, which ſeemed to anſwer ſomewhat near the truth; and made him ſo forward as to believe, that they would have an equal number of revolutions in the ſame diſtance, even tho' the motion of the boat were ſwift or ſlow between mark and mark.

Dr. *Deſaguliers* (who was frequently preſent at making the experiments of the aforeſaid invention) differed from Mr. *De Saumarez* in this particular, in regard he ſaid the forks muſt have different poſitions, according to the velocity of the veſſel to which they were fixed; and conſequently could not have an equal number of revolutions in ſwift and ſlow motion.

Whilſt Mr. *De Saumarez* was conſidering where to carry on his experiments to prove the verity of his inſtrument, and to anſwer this objection, he was introduced to the late ingenious Mr. *Samuel Molyneux*, who, as he was one of the Lords Commiſſioners of the Admiralty, and Mr. *Saumarez*'s inſtrument fell within his province, he expreſſed a deſire to ſee an experiment of it on the river of *Thames*: Accordingly, Mr. *De Saumarez* ſhewed him, and ſeveral of the principal Officers and Commiſſioners of his Majeſty's Navy, the nature and uſe of it between *London* and *Woolwich*; when he ſeemed to be of the ſame opinion with Dr. *Deſaguliers*, *viz.* whether in a certain

diſtance,

diſtance, and in different motions of the veſſel, the inſtrument could revolve equally. Hereupon he adviſed Mr. *De Saumarez* to take a trip over to *Holland*, and try his machine with the log, in his paſſage; as alſo throughly to examine the truth of the objection on the long canals in that country, where there was little or no tide or current.

Accordingly Mr. *De Saumarez* had orders to embark on board the *William* and *Mary Yatch*. This machine being fixed to the ſtern of this veſſel, he kept her run both by it and by the log. On the niceſt calculation, in his paſſage over, the difference was 2 miles and 2640 feet. At this Mr. *De Saumarez* was nowiſe ſurpriſed: For, as he knew the log to be very erroneous, and he undertook to correct the errors of it by his inſtrument (in the truth of which he might then be too forward) he was aſſured they could not agree; and therefore, he charged the difference accordingly.

Among the company on board the *Yatch*, we had a curious Gentleman, one Captain *Lynſlager*, Commander of a *Dutch* man of war, who ſeemed not a little pleaſed with Mr. *De Saumarez*'s contrivance; and no ſooner did he land in *Holland*, but he mentioned it to ſome of the higheſt rank there, whoſe curioſity induced them to deſire to ſee an experiment of this invention: Accordingly, Mr. *De Saumarez* was ſent for to the *Hague*; and on the *Canal* there, before Baron *Hop*, Baron *Waſſenaar*, Admiral *Somelſdyk*, M. *'S Graveſand* (Profeſſor of the Mathematics in the Univerſity of *Leyden*) Captain *Lynſlager*, *&c.* he run a certain diſtance in ſwift and ſlow motion; in order to ſee if the inſtrument would have an equal number of revolutions therein. In running up, it revolved 2300 times, and in coming down 2060. Here then was enough to convince Mr. *De Saumarez*, that Dr. *Deſaguliers*, and Mr. *Molyneux*, had judged truly of the fork; and more eſpecially, ſince the learned M. *'S Graveſand* joined in opinion with them; who, notwithſtanding, encouraged Mr. *De Saumarez*, by telling him his labour was not in vain; for, that the inſtrument might ſtill be of good ſervice, by making tables to rectify the different revolutions.

Tho' Dr. *Deſaguliers*, Mr. *Molyneux* and M. *'S Graveſande* did jointly agree as to this invention; yet ſtill Mr. *De Saumarez* entertained ſome ſlender thoughts, that it muſt anſwer the purpoſe, in the manner he had propoſed: For, when he conſidered, that he had two fathoms of rope out on the *Dutch Canal*, which was but five or ſix foot deep; and that the fork of his

his machine weighed about three pounds, or three pounds and a half; and was two foot and a half long, he thought it not unreaſonable to ſuppoſe, that its weight, in the ſloweſt motion of the veſſel, might occaſion it to ſtrike ground; and conſequently, impede and leſſen its motion, as alſo the number of revolutions as above: Of this Mr. *De Saumarez* had been fully ſatisfied whilſt in *Holland*; but fearing to loſe his paſſage in the *Yatch*, he was obliged to haſten over.

Not long after Mr. *De Saumarez* came to *England*, his worthy Patron Mr. *Molyneux* died; and as there was now but little hopes of his going over to *Holland* in the manner he had done before, he was notwithſtanding reſolved to take that journey at his own expence: And accordingly did ſo; where he no ſooner began his experiments, but he was convinced of the truth of the objections of the three learned Gentleman aforementioned; which plainly appears from the following Fig. 5. wherein the poſition of the fork, in five different motions of the veſſel, is repreſented.

This needs no explanation: For, it plainly appears, that the pallets will be more or leſs affected by the reſiſtance of the water, according to the poſition they are in; and therefore, the revolutions in a ſwift or ſlow motion, in the ſame diſtance cannot be equal.

Being now fully perſuaded, that the fork would not revolve equally in the ſame diſtance, and in different motions of the veſſel, he now began to repair this defect by calculating ſome tables, which render it ſtill a very uſeful inſtrument. On what foundation he formed theſe tables, there will be no need to mention; ſince he goes on to ſhew what farther improvements he has made of this inſtrument, and that it is now every way uſeful without them. And this, he thinks, he cannot better do, than by giving here the extract of a letter to Dr. *Deſaguliers*, from a learned Mathematician of *Holland*, who was ſeveral times preſent, whilſt Mr. *De Saumarez* was making his experiments on that ſide.

'Mr. *De Saumarez* having deſired me to acquaint you of the 'ſucceſs of the experiments, which I have ſeen him make of 'this machine, for meaſuring the way of a ſhip in the ſea, it is 'with pleaſure I undertake it; ſince I am fully perſuaded you 'will not be wanting to contribute all in your power to promote 'an invention ſo uſeful and advantageous as this is.

'The firſt experiment that I attended was with an iron-fork 'ſuch as the Gentleman himſelf has deſcribed in the *Philoſo-*

phical

'*phical Transactions*; when the number of revolutions were 'more in the swift than in the slow motion of the boat, on 'which we tried this instrument. This I take to be owing to 'the different inclinations of the machine; which were more 'horizontal, according as the motion of the boat was more 'swift; from whence we concluded, that it would be necessary 'to help this by some tables, calculated for the purpose: Since 'which Mr. *De Saumarez* hath accordingly formed such tables. 'But as I was not present at the experiments upon which they 'are founded, I leave you to the Gentleman himself to give 'you an account thereof.

'I have also made another experiment with Mr. *De Saumarez*, upon a new correction of his machine, which he will 'better explain to you, when you see him, than I can describe. 'Here he has contrived the first movement of his machine to 'lie horizontal under the water: And such was our success in 'this experiment, that I make no more doubt of the usefulness 'of this invention, which I look upon as very advantageous to 'navigation; since the number of revolutions here scarcely dif'fered 4 in 332, in the different velocity or motion of the 'boat: But this I must observe, that the number of revolu'tions here were greater when we moved slowest. For my part, 'I do not question, but that by a small correction, the number 'of revolutions may be always rendered proportional to the dis'tance; yet let us make no hypothesis: For, experiments of 'this machine, wherein may be had some millions of its revo'lutions, will perfectly shew the use that may be made thereof. 'In the interim I believe that Mr. *De Saumarez*'s invention may 'be, nay, ought to be, especially with this last improvement, 'infinitely preferr'd to all other methods for ascertaining the 'way of a ship in the sea, *&c.*'

Here then is the opinion of a learned Gentleman concerning Mr. *De Saumarez*'s improvement on this invention, whose eminence among the *Literati* is such, that this alone might give a sanction thereto. It is here observed, that the difference in the revolutions of Mr. *De Saumarez*'s machine, on this new method, was scarcely 4 in 332. Who then can say this difference was not owing to the different sheers in the boat on the canal? But Mr. *De Saumarez* will not go about to determine this: It remains for him now only to shew the improvement he made of the *marine surveyor*, whilst in *Holland*, which is hinted at in the letter above; and which is now brought to such perfection, that he persuades himself no very material objections can be brought

against it. The following figure shews this improvement wherein the objections of the different inclinations of the fork are now entirely removed.

A F G H (Fig. 6.) represents the fork, in the same form as the iron one described above, which differs from the other only in the materials of which it is framed; this being contrived of such as to make it equiponderous with the water, and to lie in a horizontal position; even tho' the ship or vessel, to which it is fastened, be at anchor, or under sail. H B represents a rope of a convenient length, fixt to a screw or worm at the point B which goes about six inches into an iron pin, of which B I is the length. Thro' this pipe an iron spindle passes into the aforesaid screw or worm, to which the dial C is fixed; as soon then as the vessel moves, the fork plays in a horizontal position; which moving the spindle within the iron pipe, the motion is thereby communicated to the dial, which is fitted to strike the miles or leagues the vessel runs; and let the vessel move swift or slow, the pallets A and F are equally affected; and consequently, must measure the distance sailed to a greater exactness than the iron fork is capable of, in the manner he has described above. For want of better conveniencies when in *Holland*, he had this iron pipe fixt to a thin board, which he fastened to the rudder of the vessel; but as he is now falling on a properer method to fix this iron pipe, *&c.* which he could not well do in *Holland*; since the cold weather was so far set in that it would not permit him to make more experiments than he did on that side; he hopes soon to make it appear, that the revolutions are exactly equal in this new improvement of the fork.

Mr. *De Saumarez* has been lately endeavouring to make a farther improvement in navigation, whereby he proposes to make a ship work far better to windward, than it is possible for the most weatherly one to do at present; as also to make them tack and ware in much less room than is generally done on such occasions. The advantages arising from such a projection, if it prove practicable, must be considerable: For, 1. The ship which is in danger of a lee-shore will hereby be enabled to weather the point she may want, and not be forced, in stormy weather to anchor in the very breach of the shore, and even in the jaws of destruction. Of this we have had too many melancholy instances, where several lives and fortunes have been lost; disasters of which kind, it is humbly conceived, may, in a great measure, be prevented by this invention. 2. Hence

need not fear to get the weather-gage of an enemy; For, by plying to windward much faster than he can, and by tacking and waring in much less compass, we can either leave him, or continue to engage him, as shall appear most convenient: At least we can spend the day in such a manner, as to be able to secure ourselves under the covert of the night; or if we chance to be near the land, we may hereby be enabled to gain a safe harbour. 3. By this invention the wild steerage, which is too frequently made in some ships, will be prevented; which all mariners must allow to be of service, especially in chasing, or being chas'd by an enemy; as well as in their keeping the reckoning of the ship's way, &c.

An Account of the Cinnamon-tree *in* Ceylon, *and its several Sorts.* Phil. Transf. N° 409. p. 97.

THE first and best sort of cinnamon, which grows in great plenty in *Ceylon*, and is peculiar to that island, is called by the natives *Rasse Coronde*, that is, sharp, sweet cinnamon. 'Tis this choice sort, which is exported yearly by the *Dutch East India* Company, by whom it hath been prohibited under severe penalties, that no other sort should be mixt with it.

The second sort is called *Canatte Coronde*, that is bitter and astringent cinnamon: For, the *Ceylonese*, in their language, call cinnamon in general *Coronde*, and *Canatte* signifies bitter or astringent. The bark of this tree comes off very easily, and smells very agreeably when fresh, but hath a bitter taste. It is an advantage to us that it doth not grow in great plenty hereabouts; because one might easily mistake it for a better; as indeed, in general, it requires a good deal of skill and attention so to distinguish the cinnamon trees from each other, as not to choose now and then a worse sort for the best. The root of this second tree yields a very good sort of camphire.

The third sort is call'd *Capperoe Coronde*, that is, camphorated cinnamon; because it has a very strong smell and taste of camphire. It grows plentifully enough in the island, but not in the eastern parts thereof: However, they find means now and then to send it over privately, and sell it to the *Danes* and *English*, who come to trade on the coast of *Cormandel*. Besides, there is a sort of a *canella*, on the continent of *India*, about *Goa*, which is very like this sort of cinnamon-tree, tho' it have nothing of the true cinnamon. The same sort of *canella* agrees in several particulars with the *canella Malabarica sylvestris*, a wild cinnamon-tree growing upon the coasts of *Malabar*. And

tho' with regard to the ſhape of the tree, aud to the outward appearance of the bark and leaves, there be very little difference to be obſerv'd between theſe two ſorts of *canella*, and the beſt ſort of cinnamon, yet the latter is vaſtly ſuperior in richneſs, virtues, and ſweetneſs,

The fourth ſort is call'd *Welle Coronde*, that is, the ſandy cinnamon; becauſe upon chewing it, one feels bits of ſand, as it were, between the teeth; tho' in fact there be nothing ſandy in it, The bark of this tree comes off eaſily enough, but is not ſo eaſily roll'd up into a fibular form, as other ſorts of cinnamon are, being apt to burſt open and unfold itſelf. It is of a ſharp and bitteriſh taſte, and the root of it yields but a ſmall quantity of camphire.

The fifth ſort is called *Sewel Coronde*; *ſewel* in the *Ceyloneſe* language ſignifies mucilaginous or glutinous. This ſort acquires, in drying, a very conſiderable degree of hardneſs, which upon chewing of it ſufficiently ſhews itſelf. It hath otherwiſe but little taſte, and an ungrateful ſmell; but the colour of it is very fine; and it is not many years ſince the author firſt took notice, that the natives mix a good deal of this mucilaginous cinnamon with the beſt ſort, the colour of both being very much alike; excepting only, that in the good ſort there appear a few yellowiſh ſpots towards the extremities.

The ſixth ſort is call'd *Nicke Coronde*; the tree which bears it having a good deal of reſemblance to another tree, the *Ceyloneſe* call *Nieke Gas*, and the fruit it bears *Nieke*: The bark of this ſort has no manner of taſte or ſmell, when taken off, and is us'd by the natives only in phyſick: For, by roaſting of it they obtain a water and oil, with which they anoint themſelves, thinking thereby to keep off all ſorts of noxious fumes, and infections in the air. They likewiſe expreſs a juice out of the leaves of it, which they ſay cools and ſtrengthens the brain, if the head be rubb'd with it.

The ſeventh ſort is call'd *Dawel Coronde*, that is, drum cinnamon, in *Low Dutch Trommel Canell*: The reaſon of this appellation is, becauſe the wood of this tree, when grown hard enough, is light and tough, and that ſort, of which the natives make ſome of their veſſels and drums, which they call *Dawen*. The bark is taken off, when the tree is ſtill growing, and is of a pale colour: The natives uſe it in the ſame manner with the ſixth ſort.

The eighth ſort is call'd *Catte Coronde*, that is the thorny or prickly cinnamon; *Catte* in the *Ceyloneſe* language, ſignifying a thorn

thorn or prickle, for this tree is very prickly. The bark in ſome meaſure reſembles cinnamon; but the leaves differ very much therefrom; and the bark itſelf hath nothing either of the taſte or ſmell of cinnamon. The natives uſe the root, bark and leaves of this tree in phyſick, applying them in form of cataplaſms, to tumours and ſwellings from a thick corrupt blood, which they ſay it cures in a ſhort time.

The ninth ſort is call'd *Mael Coronde*, or the flowering cinnamon, becauſe this tree is always in bloſſom. The flowers come neareſt to thoſe of the firſt ſort; but they bear no fruit, which the other do. The ſubſtance of the wood becomes never ſo ſolid and weighty in this, as in the other cinnamon trees abovementioned, which have ſometimes eight, nine, or ten, feet in circumference. If this ever-flowering cinnamon tree be cut, or bored, a liquid water will iſſue out of the wound, as it does out of the *European* birch-tree; but it is of no uſe, any more than the leaves or bark are.

The inhabitants of *Ceylon* ſay, there is ſtill another ſort of cinnamon, which they call *Toupat Coronde*, or the three leav'd cinnamon. It does not grow in that part of the country which the *Dutch East India* Company is poſſeſs'd of, but higher up towards *Candia*; this the author never ſaw.

All the ſeveral ſorts of cinnamon trees, muſt grow a certain number of years, before the bark be fit to be taken off: With this difference however, that ſome of the trees of the ſame ſort, as for inſtance of the firſt and beſt, will ripen two or three years ſooner than others, which is owing to the difference of the ſoil they grow in: Thoſe, for inſtance, which grow in valleys, where the ſoil is a fine white ſand (and there are ſeveral ſuch valleys in the iſland of *Ceylon*) will in five years time be ready; whereas thoſe, that ſtand in a wet ſlimy ſoil, muſt have ſeven or eight years to grow before they are ripe enough. Again thoſe trees are later, which grow in the ſhade of other larger trees, whereby the ſun is kept from their roots: And hence likewiſe it is, that the bark of ſuch trees hath not that ſweetneſs and agreeable taſte, obſervable in the bark of thoſe trees, that grow in a white ſandy ſoil; where with little wet they ſtand full expos'd to the ſun; but is rather of a bitteriſh taſte, ſomething aſtringent, and ſmells like camphire.

For, by the heat of the ſun's rays the camphire is rendered ſo thin and volatile, that it riſes up and mixes with the juices of the tree; where it undergoes a ſmall fermentation; and

then

then rising still higher between the substance of the wood, and the thin inner membrane of the bark, it is at last so effectually diffus'd thro' the branches and leaves, that there is not the least trace of it to be perceiv'd any where. Mean while that thin and glutinous membrane, which lines the bark on the inside between it and the substance of the wood, attracts and sucks in all the purest, sweetest, and most agreeable particles of the juice, leaving the thick and gross ones, which are push'd forwards and serve to nourish the branches, leaves, and fruit.

What the author here mentions is from his own observations, and he has often had occasion to prove this fact to curious persons by the things themselves: For, if the bark be fresh taken off, that juice which remains in the tree hath a bitterish taste, not unlike that of cloves: On the contrary, if you taste the inner membrane of the bark, when fresh taken off, you will find it most exquisitely sweet, and exceeding agreeable to the taste; whereas the outer part of the bark differs but very little in taste from the common trees; which shews plainly that all its sweetness is owing only to the inner membrane. But when the bark is laid in the sun in order to be dried and wound up, this oily and agreeable sweetness of the inner membrane diffuses itself throughout the whole outer part of it (which however has been first stripped, whilst still upon the tree, of its outermost greenish coat) and impregnates it so strongly, as to make the bark a commodity, which, for the fragrancy of its smell, and the sweetness of its taste, is coveted all over the world.

The bark may be taken off from trees, which have stood 14, 15, or 16 years, after they are come to maturity, according to the quality of the soil they stand in: But after that time they lose, by degrees, their taste and agreeable sweetness, which makes the bark have more of the taste of camphire: Besides, the bark is then grown so thick, that if it be laid in the sun, it will no longer shrink and wind itself up, but remain flat.

And here it may be thought a fit subject of enquiry, how it comes to pass, that considering what vast quantities of cinnamon have been exported from this island, and sold all over the world; there are still such numbers of good trees fit to be bark'd, remaining in the island, and growing there every year? Now in order to solve this question, several authors who have describ'd the island of *Ceylon*, have committed a considerable mistake, when they assur'd their

reader

readers, that when the bark hath been ſtripp'd off the tree, it grows again in four or five years, and becomes fit to be ſtripp'd a ſecond time: But this aſſertion is entirely contrary to the courſe of nature and obſervation: Nor does he believe, that there is any one tree whatever in any part of the world, which, if it were entirely ſtripp'd of its bark, could ſubſiſt and grow any longer: That part at leaſt, where the bark hath been taken off, will quickly grow dry, and ſo die away; but the root in the mean while remains entire, and in good condition; and this ſhews the reaſon, why there is ſuch a number of trees fit to be bark'd every year: For, tho' the cinnamon trees, after the bark hath been once taken off, are cut down to the very root, as in *Europe* they do oak, birch trees, alders and willows; yet the roots will quickly puſh forth new ſhoots, which will grow in a ſhort time, namely, in 5, 6, 7, or 8 years; ſome ſooner and ſome later, and then yield their quantity of bark. Hence it appears, how far the old roots are inſtrumental to the growth and plenty of cinnamon trees; as alſo the fruit that falls from the trees very much contributes to the ſame end; and it is particularly owing to a certain kind of wild doves, which, from their feeding on the fruit of the cinnamon-tree, are call'd *cinnamon-eaters*, that theſe trees grow ſo plentifully in this iſland: For, the doves (when they fetch food for their young ones) in their flights, diſperſe vaſt quantities of the fruit over the fields; which occaſions the riſe of ſeveral thouſand young trees, which may be ſeen along the roads in ſuch quantities, that they look like a little wood.

The oil, obtain'd from it by fire, is reckoned one of the ſtrongeſt cordial medicines: The camphire, which comes out of the root, is likewiſe of great uſe in ſeveral diſtempers; as are alſo the oil of camphire (a very coſtly thing) the leaves of the tree, and the oil diſtill'd from them; and laſtly the fruit with their oil. In ſhort, there is no part of the cinnamon-tree, but what is of ſome ſingular uſe in phyſick.

The

The Sequel of the Bills *of* Mortality *for the Years* 1724 *and* 1725; *by Dr.* J. G. Scheuchzer. Phil. Tranf. N° 409. p. 110.

The following is a lift of thofe that were born and buried in *Breflaw* in 1724.

Perfons born	
From the 25th to the 31ft of December, 1723	17
January, 1724	115
February	101
March	129
April	79
May	122
June	99
July	129
Auguft	177
September	141
October	134
November	126
1ft December to the 24	97
Sum Total	1466

Among the dead were		
Married men		231
Married women		148
Widows and widowers		154
Batchelors		57
Maidens		66
Children to 10 years of age	boys	417
	girls	326
Stillborn	boys	36
	girls	31
Total		1466

Chriftened.	
Males	709
Females	613
Total	1322

Married 386 pair.

In *Vienna* were buried 5524

Among whom	years of age	
3	of	90
2		92
2		93
5		95
2		96
1		97
3		98

Among whom	Years of age	
1	of	99
2		100
3		101
2		103
1		106
27	of 90 and upwards	

Chriftened 4427.

In *Lubau.* Buried 135. Chriftened 166. Married 38 pair.

Chriftened. Boys 107. Girls 116. Total 223.

Buried

At *Lauban*.

Buried	Married men	29	Buried	Infants	23
	Married women	19		Stillborn	17
	Batchelors	7		Widows and widowers	18
	Maidens	15			
	Children: Boys	68			
	Children: Girls	61		Total	257

Among which there died 97 children of the ſmall-pox. There were married 73 couple.

In *Dreſden* christened	boys	715	Baſtards	boys 60, girls 70	Total 1557
	girls	712			
Buried	Married men	161	Children	boys	522
	Married women	151		girls	508
	Widowers	35	Stillborn	boys	58
	Widows	143		girls	40
	Batchelors	72			
	Maidens	71		Total	1761

Buried 204 more than were chriſtened. Married couple 413.

In *Leipſic*. *Chriſtened in*

January	79	*in* July	78
February	71	Auguſt	94
March	99	September	79
April	78	October	79
May	74	November	50
June	69	December	63
		Total	913

Chriſtened in all 913. Boys 491. Girls 422; among whom were 3 children at one birth, born *October* 21. One being alive, the other two ſtill-born; beſides 14 twins.

Buried in	January	55	Married men	122
	February	78	Married women	81
	March	87	Batchelors	51
	April	109	Maidens	44
	May	100	Children: boys	234
	June	74	Children: girls	210
	July	90	Women in child-bed	17

August	86	Infants { boys	59
September	77	{ girls	28
October	73	Stillborn { boys	33
November	70	{ girls	28
December	64	Widows and widowers	54
Total	961	Total	961

Married couple 276.

In *Erfurt*.		In *Salfeld*.	
Christened	659	Christened	11
Buried	612	Buried	9
Married couple	188	Married couple	2

In *Gera* buried.

Married men	28
Married women	15
Widowers	4
Widows	22
Batchelors	6
Maidens	4
Children { boys	76
{ girls	42
Total	197

including 10 still-born

Christen'd { boys	15
{ girls	14
Total	29

Including 6 twins, one of which was stillborn.
Married couple 82

In *Berlin*.

Christened	2798
Buried	249
Married couple	86

In all the King of *Prussia*'s dominions, as follows.

	Born	*Married couple*	*Buried*
In the kingdom of *Prussia*	21685	4611	1368
Churmarck Brandenburg	19507	5019	1294
New Marck	7044	1838	428
Dukedom *Magdeberg* and County of *Mansfeld*	8584	2073	603

Dukedom

Dukedom of *Cleves* and county of *Marck*	6949	2071	7182
Dutchy of *Pomerania*	8168	1949	5670
Principality of *Halberstad*	2889	781	2326
County of *Hohestein*	593	171	399
Principality of *Minden*	2203	684	2030
County of *Ravensberg*	2417	680	2166
Principality of *Mers*	426	166	529
Dukedom of *Geldren*	1861	420	1860
County of *Teklenburg*	497	146	517
County of *Lingen*	769	255	647
Lauenburg and *Butow*	607	133	361
French colonies	711	185	546
Total	84910	21182	61112

Among the christened were 2215 bastards. Among the dead 66, who liv'd to 90 and upwards, as far as 100 years of age.

In *Regensburg* among the protestants.

Christen'd boys	172
Christen'd girls	126
Total	298

Among which were 5 twins.

Buried.

Married men	34
Married women	46
Including 13 widows and four that died in child-bed.	
Batchelors	13
Maidens	7
Children boys	79
Children girls	110
Total	289

Married couple 68.

In *Amsterdam.*

Buried	7622
Married	2294

In *Venice.*

Buried	4590
Born	5046

In *Copenhagen*.

Christen'd	boys	1306
	girls	1183
	Total	2489

Buried Men	486
Women	343
Boys	991
Girls	931
Total	2751

Buried 262 more than were born, and 837 more than there died in *Venice* the year before. This extraordinary mortality being ascrib'd to the small-pox being very rife.

Married couple 748.

In *Dantzick*. Christened 1999. Married couple 488. Buried 1872 or 377 more than the year before.

The following are the bills of mortality for the year 1725.

At Breslaw.	
From Dec. 25 to 31, 1724	30
January	125
February	115
March	129
April	166
May	122
June	106
July	121
August	107
September	115
October	141
November	97
December 1 to 24	67
Total	1441

Among which were.

Married men		259
Married women		153
Widows and widowers		158
Batchelors		64
Maidens		58
Children to 10 years of age	boys	364
	girls	306
Stillborn	boys	44
	girls	35
Total		1441

Christen'd	boys	664
	girls	675
	Total	1339

Married 363 couple

In

In *Vienna*. Buried

Men	1007
Women	1433
Boys	1865
Girls	1560
Total	5865
Christened	4708

Among which were

8 of	90
1	91
3	92
2	93
1	94
3	95
2	96
2	98
1	99
3	100
1	102
1	103
1	106

29 of 90 and upwards.

In *Dresden* christened

Boys	758
Girls	714
Bastards { Boys	68
Bastards { Girls	60
Total	1600

Buried married Men	225
Married women	174
Widowers	36
Widows	65
Batchelors	99
Maidens	167
Children { Boys	478
Children { Girls	398
Total	1642
Among which stillborn { Boys	53
Among which stillborn { Girls	33
Married couple	519

In *Leipsic* christened boys 478

1 *Jew* 20 years of age

Girls	461
Total	940
Among which were posthumous births	6
Twins	12
Bastards	141

And among them 3 twins.

Married 260 couple.

Buried

Married men	113
Married women	75
Batchelors	49
Maidens	34
Boys	165
Girls	106
Women in childbed	10
Children { Boys	58
Children { Girls	51
Stillborn { Boys	45
Stillborn { Girls	24
Widows and widowers	77
Total	807

In

In *Erfurt* buried 617. Christened 624. Married 183 couple.

At *Coburg* christened		Buried	
Boys	105	Married men	35
Girls	101	Married women	35
		Women in childbed	2
		Batchelors	4
Total	206	Maidens	10
Among which were 3 twins and eight bastards.		Children	96
		Total	182

Six of the children still-born.

Which according to the months is as follows.

	Christen.	Bur.		Christ.	Bur.
January	17	22	July	12	18
February	27	13	August	10	14
March	24	18	September	21	13
April	18	20	October	23	15
May	16	11	November	11	9
June	15	10	December	12	19
	117	94		89	88
				117	95
			Total	206	183

Married couple 46

In *Ragensberg* among the Protestants.

Christened			Buried	
	Boys	142	Married men	42
	Girls	126	Married women	39
			Batchelors	13
	Total	268	Maidens	11
Among which twins		4	Children { boys	58
			Children { girls	50
			Total	213

Married couple 80.

In *Franckfort* on the *Main* Christened Boys 346
Girls 385

731 including 10 twins, 4 posthumous births, 1 jew, 2 foundlings and 22 bastards.
Buried 843.

In *Sachsenhausen* Christened Boys 87
Girls 79

166 including 2 twins and 9 bastards.
Buried 168

Married pair in *Franckfort* and *Sachsenhausen* 209.

Marriages at *Amsterdam* in the reformed church in

1724	2294
1725	2249

In *Venice*		In *Dantzic*	
Born	4836	Christened	2012
Buried	4816	Buried	1678
		Married couple	46

An Account of an Earthquake *at* Boston *in* New England; *by Mr.* Benjamin Colman. Phil. Trans. N° 409. p. 124.

THE earthquake came suddenly on in the night *Oct.* 29, 1727 between 10 and 11 o'clock, in a very still and fair evening; the stars so bright and glittering, that several people had taken particular notice of them; and one or two persons that had been in places subject to earthquakes had said transiently, that if we had been used to have them, they should expect one. This was the only general symptom of it's approach Mr. *Colman* heard of, namely, the most serene sky and calm air that ever was known, not a cloud in the sky, nor scarce a breath of wind stirring. And tho' this be not universally a symptom of earthquakes coming on places; yet so far as he could inform himself, it has frequently and for the most part been observed.

It

It was ſo in the dreadful ſhock in *Jamaica* upwards of 30 years before: And a very ingenious friend of Mr. *Colman's* informed him, that after that ſhock, which was followed with ſeveral tremors and leſſer concuſſions, he could from day to day judge by the face of the ſky and air, whether there would be any tremor of the earth. If there were any cloud hanging over the mountainous part of the iſland, there was no ſhake that day; but if all were ſerene and fair, he expected one, and it ſeldom fail'd of happening.

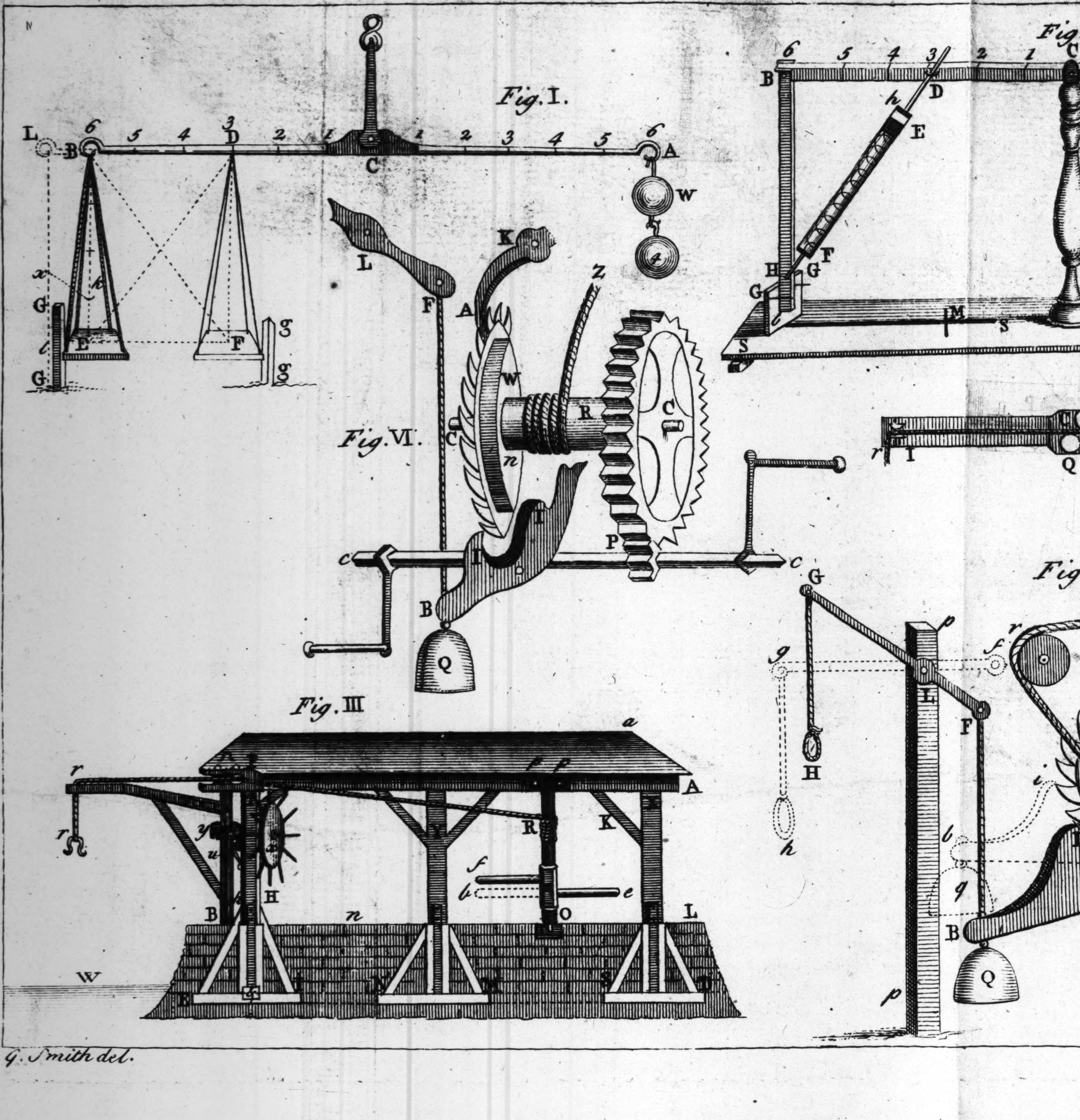
Yet it was not found ſo, in the after tremors, which frequently returned for ſome months after this laſt great ſhock; and at times for 9 months.

The town of *Newbury*, at the mouth of *Merrimack* river, about 40 miles north-eaſt from *Boſton*, is the place that ſeems to have been the centre of the ſhock and tremors felt: There the earth opened, and threw up ſeveral loads of a fine ſand and aſhes, mixt with ſome ſmall remains of ſulphur; ſo that taking up ſome of it between the fingers and dropping it into a chaffing diſh of bright coals, in a dark place, once in three times the blue flame of the ſulphur would plainly ariſe, and yield a very ſmall ſcent. By this it ſeems evident, that it was a ſulphureous blaſt which burſt open the ground, and threw up the calcined bituminous earth. The family neareſt to this eruption (it being in that part of the town where the houſes lie at a diſtance from each other) were in the utmoſt conſternation; the ſhock and noiſe being much more terrible upon them than upon others: And yet at 40 miles diſtance, and upwards, it was very dreadful and aſtoniſhing.

After the firſt and great ſhock five or ſeven ſmall tremors were felt, that night and next morning; but theſe and other following rumblings and tremors were louder and greater at *Newbury*, and the adjacent places: But yet at times at 40 miles diſtance were felt and heard ſome of the greater ones, both by day and night.

Mr. *Colman* had the following account from Mr. *Lowel* miniſter in *Newbury*.

'As to any previous notices of the approach of the earth'quake, I cannot find any thing to be depended on. The prog'noſtications that have been among us have all fail'd; ſuch as 'an unuſual brightneſs of the ſky, twinkling of the ſtars, *&c.* 'I certainly know that we have heard the rumblings in all wea'thers, cloudy, foggy, rainy, ſnowy, clear, cold, hot, mode'rate, windy, calm, *&c.* indifferently; and at all hours of the 'day



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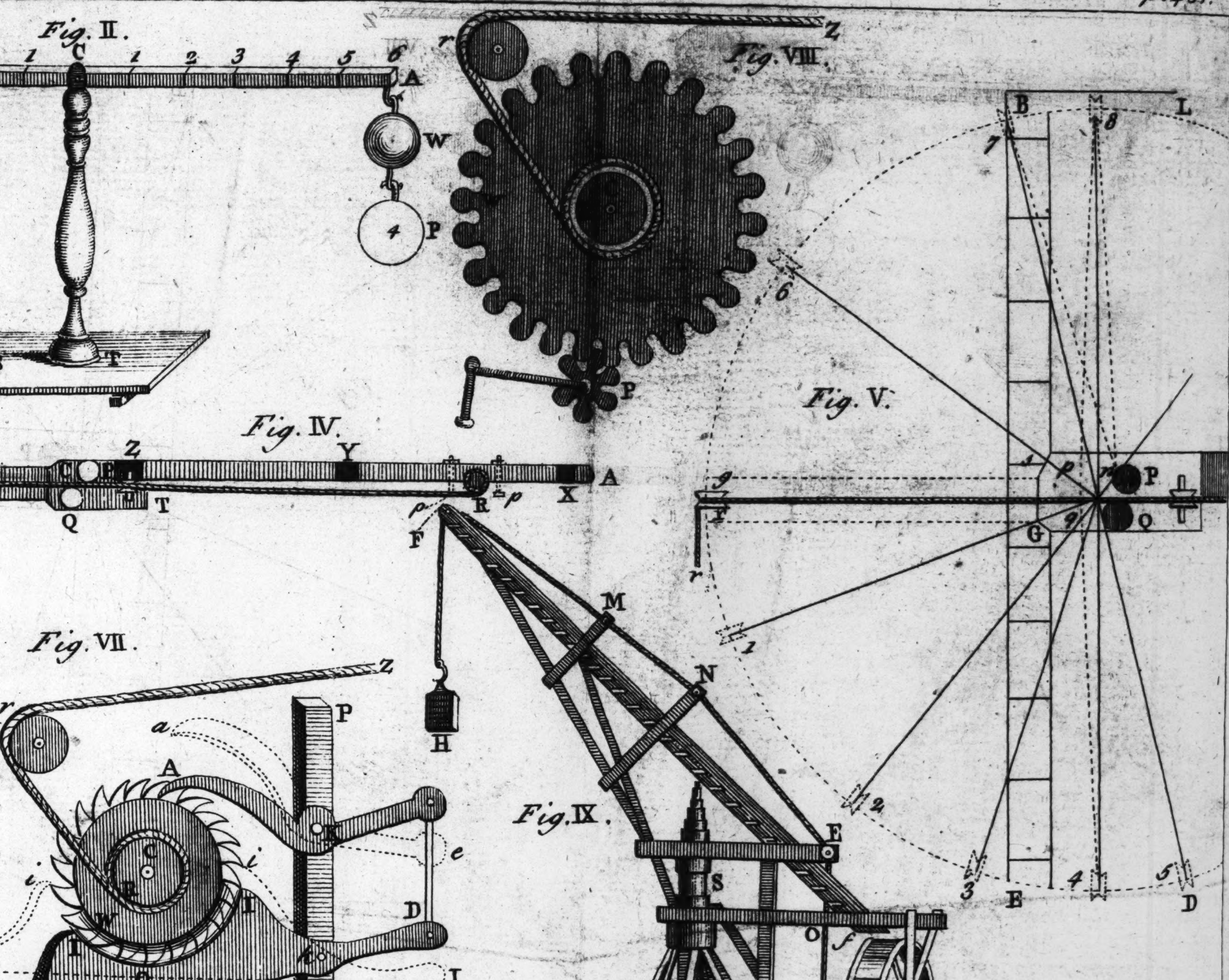
Fig. II.
Fig. VIII.
Fig. IV.
Fig. V.
Fig. VII.
Fig. IX.
Hulett Sculp

'day and night (tho' we heard these rumblings oftener in the 'night during winter, as I think, and since more commonly in 'the day) as also when the wind has been at any point of the 'compass, and at all times of tide; and as to the moon, 'equally when she was nearer or farther from her change or 'full; neither in any particular weather, nor on any observable 'occasion were the shocks greater, or rumblings louder.

'As to any alterations in the air or water after a shock, I 'could never discern any; particularly as to the wind being 'raised after a shock, when it was calm before, which some re-'ported, but I could never perceive the least difference.

'One thing I may add here, which is very remarkable, and 'may be depended on. That about the middle of *April*, that 'fine sand, which was thrown up in several places in this pa-'rish at the first great shock *Oct.* 29, had a very offensive 'stench, nay, was more nauseous than a putrifying corps; yet 'in a very little while after it had no smell at all. How long 'it was before it begun to have this stench I am not certain; I 'know it had it not at first; and I believe it was covered with 'snow till a little while before: There is no smell now: There 'has been no opening of the ground, throwing up of sand, 'stopping or breaking out of springs, *&c.* as at first. *New-'bury* (as also the adjacent towns) is a spot of ground very 'much inhabited and continually travelled over; and as to the 'sea, contiguous to these parts it is full of our coasters day and 'night; but no sensible eruptions or evacuations (that I hear 'of) have been observed either on land or water.'

Boston weekly news Letter. Sept. 5, 1728. We hear from *Newbury* and *Rawley*, that they felt a shake of the earth on *Thursday* last, about four in the morning, the noise much like thunder.

A Proposition on the Balance (not taken notice of by Mechanical Writers) *explained and confirmed by an Experiment; by Dr.* Desaguliers. Phil. Transf. N° 409. p. 306.

Theor. 6. AB (Fig. 1. Plate XIII.) represents a balance, on which is supposed to hang at one end, B, the scale E with a man in it, who is counterpoised by the weight W hanging at A, the other end of the balance: If such a man, with a cane or any rigid streight body, push upwards against the beam any where between the points C and B (provided he do not push directly against B) he will thereby make himself heavier, or overpoise the weight W; tho' the stop G G hinders the

the ſcale E from being thruſt outwards from C towards G G; And likewiſe if the ſcale and man, ſhould hang from D, the man by puſhing upwards againſt B, or any where between B and D (provided he puſh not directly againſt D) will make himſelf lighter, or be overpoiſed by the weight W, which before only counterpoiſed the weight of his body and the ſcale.

If the common centre of gravity of the ſcale E, and the man, ſuppoſed to ſtand in it, be at *k*; and the man by thruſting againſt any part of the beam, cauſe the ſcale to move outwards, ſo as to carry the ſaid common center of gravity to *kx*, then inſtead of B E, L I will become the line of direction of the compound weight, whoſe action will be increaſed in the ratio of L C to B C. This is what has been explained by ſeveral writers of mechanics: But no one, that the Dr. knows of, has conſidered the caſe, when the ſcale is kept from flying out, as here by the poſt G G, which keeps it in its place, as if the ſtrings of the ſcale were become inflexible. Now to explain this caſe; let us ſuppoſe the length B D of half the arm B C to be equal to three foot; the line B E to four foot; the line E D of five foot to be the direction in which the man puſhes; D F and F E to be reſpectively equal and parallel to B E and B D, and the whole or abſolute force with which the man puſhes equal to (or able to raiſe) 10 ſtone: Let the oblique force E D (= 10 ſtone) be reſolved into the two forces E F and E B (or its equal F D) whoſe directions are at right angles to each other, and whoſe reſpective quantities (or intenſities) are as 6 and 8; becauſe E F and E B are in that proportion to each other and to E D. Now ſince E F is parallel to B D C A the beam, it does no ways affect the beam to move it upwards; and therefore, there is only the force, repreſented by F D, or 8 ſtone to puſh the beam upwards at D. For the ſame reaſon, and becauſe action and reaction are equal, the ſcale will be likewiſe puſhed down at E with the force of 8 ſtone. Now ſince the force at E pulls the beam perpendicularly downwards from the point B, diſtant from C the whole length of the arm B C, its action downwards will not be diminiſhed; but may be expreſſed by $\overline{8 \times BC}$: Whereas the action upwards againſt D will be half loſt, by reaſon of the diminiſhed diſtance from the center, and is only to be expreſſed by $8 \times \frac{BC}{2}$; and when the action upwards to raiſe the beam is ſubſtracted from the action downwards to depreſs it, there will ſtill remain four ſtone

ſtone to puſh down the ſcale; becauſe, $\overline{8 \times BC} - 8 \times \frac{BC}{2} = 4BC$; conſequently a weight of 4 ſtone muſt be added at the extremity A to reſtore the *æquilibrium*: Therefore, a man, *&c.* puſhing upwards under the beam between B and D, becomes heavier. Q. E. D. On the contrary, if the ſcale ſhould hang at F from the point D, only three feet from the center of motion C; and a poſt $g\,g$ hinder the ſcale from being puſh'd inwards towards C; then if a man in this ſcale F puſh obliquely againſt B with the oblique force above-mentioned; the whole force, for the reaſons before given (in reſolving the oblique force into two other forces, acting in lines perpendicular to each other) will be reduced to 8 ſtone, which puſhes the beam directly upwards at B; while the ſame force of 8 ſtone draws it directly down at D towards F. But as CD is only equal to $\frac{1}{2}$ CB; the force at D, compared with that at B, loſes half its action; and therefore can only take off the force of 4 ſtone from the puſh upwards at B; and conſequently the weight W at A will preponderate; unleſs an additional weight of 4 ſtone be ſuſpended at B. Therefore a man, *&c.* puſhing upwards under the beam between B and D becomes lighter.

Schol. 1. Hence knowing the abſolute force of the man that puſhes upwards (that is, the whole oblique force) the place of the point of truſion D, and the angle formed by the direction of the force with a perpendicular to the beam at the ſame point, we may have a general rule to know what force is added to the extremity of the beam B, in any inclination of the direction of the force, or place of the point D.

Rule for the firſt caſe. Firſt, find the perpendicular force by the following analogy, whoſe demonſtration is known to all that underſtand the application of oblique forces.

As the radius :
To the right ſine of the angle of inclination : :
So is the oblique force :
To the perpendicular force.

Then the perpendicular force, multiplied into the length of the arm BC, *minus* the ſaid force, multiplied into the diſtance CD, will give the value of the additional force at B, or of the weight, required to reſtore the *æquilibrium* at A.

Or to expreſs it algebraically; let of expreſs the oblique force, pf the perpendicular force, and x the force required, or value of the additional weight at A to reſtore the *æquilibrium.*

$$DE : DF :: of : pf$$

$$\overline{pf \times BC} - \overline{pf \times DC} = x$$

The ſame rule will ſerve for the ſecond caſe; if the quantity found be made negative, and the additional weight, ſuſpended at B. Or having found the value of the perpendicular force, the equation will ſtand thus $-\overline{pf \times BC} + \overline{pf \times DC} = -x$; and conſequently, the additional weight muſt be ſuſpended at B; becauſe $-x$ at A is the ſame as $+x$ at B.

Schol. 2. Hence it likewiſe follows, that if in the firſt caſe, the point of truſion be taken at C, the force at B (or force whoſe value is required) will be the whole perpendicular force; becauſe C D is equal to nothing: And if the point D be taken beyond C towards A; the perpendicular force puſhing upwards at that point, multiplied into D C, muſt be added to the ſame force, multiplied into B C, that is $\overline{pf \times BC} + \overline{pf \times DC} = x$.

The machine Dr. *Deſaguliers* made uſe of to prove this experimentally was, as follows.

The braſs balance A B (Fig. 2.) is 12 inches long, moveable upon the centre C, with a perpendicular piece B *b* hanging at the end B, and moveable about a pin at B, and ſtopped at its lower end *b* (by the upright plate G G) from being thruſt out of the perpendicular by the puſhing pipe F E, whoſe lower point being put into a little hole at H, the upper wire or point (when put into another little hole under the beam at D) is by means of the worm-ſpring E F, preſſing againſt the plug E to drive forwards the ſaid wire *b* D, made to puſh the ſaid beam upwards with the force of the ſpring: T S S is a ſtand, to which is fixt the pillar T C that ſuſtains the balance; and it has alſo a ſlit S S, to receive a ſhank of the moveable plate G G, to be fixt in any part of the ſlit by a ſcrew underneath.

Experiment. Hang on B b, as in the Fig. Then let E F be applied to the hole H in ſuch a Manner, that its upper wire h D k may go thro' a little loop at D, ſo as not to thruſt the beam upwards, but be in the ſame poſition as if it did, that by hanging on the weight W, the arm B C with B b and F E may be counterpois'd; and then the action againſt D and H may be eſtimated without the weight of the puſhing pipe.

Then

Then drawing down the end of the wire *k*, thrust it into the little hole under D; and B will be so pull'd downwards, as to require the additional weight of four ounces to be hung on at A to restore the *æquilibrium*, when B H is four Inches, B D three inches, and the whole force of the spring equal to 10 ounces.

It need not here be said, that for explaining the second case, B b is to be suspended at D, with the plate G G fixt to stop it at the place M to keep it from being push'd towards T; and that the upper end of G F E D k must push into a hole, made under B; in which case, the weight B must be suspended at B to restore the *æquilibrium*.

To shew experimentally that the force, which the spring exerts in this oblique trusion, is equal to 10 ounces: Take the beam A B which weighs four ounces from its pedestal C T; and having suspended at each end A and B, three ounces, support it under its centre of gravity by the pushing pipe E F, set upright under it; and you will find that the beam with the two weights will thrust in the wire k h as far as h, the place to which the oblique trusion drives it.

An aurora borealis *at* Redbridge *near* Southampton, Oct. 13. 1728. *by Mr.* Derham. Phil. Transf. N°. 410. p. 137.

ABout eight o'clock in the evening *Oct.* 13. at *Windsor* was observ'd a considerable streaming in the north, with such bright lances and columns, as usual: But at *Redbridge* there appear'd none such; only in the north Mr. *Derham* observ'd a large, thick, black bank of vapours; the top reaching about 20° above the horizon; without any convexity or curvature, as is usual in most of the streamings he had seen; but instead of that, the upper part was indented in several places, with long black pyramids, somewhat resembling the streams of the *lumen boreale*; the edges of which were gilded with lucid rays of the streaming colour: And all over the clouds or vaporous bank, he discovered a great commotion or disturbance behind them; as if something were rolling or tumbling. The end of all these appearances he expected would have been streamings: But in less than an hour, the clouds (which had been pretty still) began to move to the S. W. and at last obscured the whole hemisphere; which before was all clear enough (excepting towards the north) to shew the Stars, tho' overcast with vapours, like a thin fog, a little inclining to red.

A

A remarkable conformation of the urinary parts; by Mr. John Budgen, Phil. Transſ. N°. 410. p. 138. *Tranſlated from the* Latin.

IN 1711 was born at *Ockley* in *Surry* a female child, on whoſe back, about the inferior *vertebræ*, appear'd an indolent tumour, of the colour of the skin, and bigneſs of a large pigeon-egg, that grew up to ſuch a ſize with the child, that when ſhe was about nine or ten years of age, it exactly reſembled a calf's bladder, when blown up, but without a neck: In 1728 it was as big as an ox's bladder. On the 29th of *Jan.* 1728-9, the tumour broke as ſhe lay in bed, from which there iſſued a large quantity of liquor, like urine. Upon narrowly ſruveying it, Mr. *Budgen* found the tumics, the mucous matter on the inſide, the ureters, veins and arteries, entirely the ſame as is common in the bladder; and there was ſome communication with the internal parts by a *foramen* in the *vertebræ*, thro' which one's little finger might enter into the *abdomen*, and which receiv'd the aforeſaid veſſels. On the ſecond of *Feb.* 1728-9, the young woman died; and had the body been open'd, Mr. *Budgen* believes he might have found the neck of the bladder in the *abdomen*, but no bladder. For, after the tumour broke ſhe did not make water ſo much as once.

An Eclipſe of the Moon at Caſtle Dobbs *near* Carricfergus *in* Ireland, Feb. 2, 1728-9. *by Mr.* Dobbs, Phil. Tranſ. N°. 410. p. 140.

Apparent Time. P. M. H	′	″	Phaſes
6	27	0	The *penumbra* obſerv'd.
	29	30	The Moon's limb immerged.
	33	0	The eaſtern limit of *Palus Mareotis* immerged.
	35	50	*Mons Climax* immerged.
	42	40	*Mons Porphyrites* immerged.
	50	0	*Inſula Melis* immerged.
	52	40	*Mons Ætna* immerged.
	54	20	*Inſula Sardinia* immerged.
	56	20	*Inſula Rhodus* immerged.
	58	0	*Inſula Corſica* immerged.
7	0	30	*Mons Sinai*'s eaſtern limit immerged,
	2	0	*Mons Sinai* entirely immerged.
	6	50	*Inſula Beſbicus*'s eaſtern limit immerged.

Appa-

Apparent time. P. M.			Phases
H.	′	″	
7	11	50	*Mons Horminius* immerged.
	13	50	*Promontorium Acherusium* immerged.
	22	0	The eastern limit of *Mare Caspium* immerged.
	24	20	The eastern limit of *Palus Mæotis* immerged.
	27	0	*Palus Mæotis* entirely immerged.
	30	15	The moon entirely immerged.
9	8	30	The moon's eastern limb near *Mons Acabe* emerged.
	13	0	*Palus Mareotis* emerged.
	15	0	*Mons Climax* emerged.
	23	30	*Mons Porphyrites* emerged.
	29	10	*Mons Sinai* emerged.
9	33	30	*Mons Ætna* emerged.
	49	0	*Insula Besbicus* emerged.
10	1	0	The eastern limit of *Mare Caspium* emerged.
	5	0	*Palus Mæotis* emerged.
	10	0	The *penumbra* observ'd; the moon's limb emerging.
	11	0	The limb evidently emerged.
			From the beginning to the end of the eclipse $3^h\ 44'\ 0''$.
			Total eclipse $1^h\ 38'\ 15''$

Of the Use of cold Water *in* Fevers, *&c. by Dr.* Cyrillus. Phil. Transf. N° 410. p. 142. *Translated from the* Latin.

THE use of cold water and other cold liquors in fevers is no modern practice, but what was pretty common among the most ancient physicians: For, in the hotter fevers, after the state of the paroxysm, they allow'd the plentiful use of water, or other cooling liquors; by which the rage of the fever abating, the patient was frequently thrown into a critical sweat: But to cure fevers by the use of water only mixt with snow, and that in large quantities for several days together, without any other medicine or food, is what Dr. *Cyrillus* takes to be entirely new; and yet this was attempted at *Naples* a few years before, not only in an uncommon, but too daring a manner. The method (as was reported) was communicated in a rude, undigested small treatise, brought from *Spain*; and by it, he was surpris'd to see, that some (contrary to

to all expectations) were ſnatched from the very jaws of death. Cautious phyſicians were at firſt ſtartled at the practice; but encouraged by repeated and ſucceſsful trials, they at length became bolder, and what ſome people attempted at random, and without conſidering either the nature or time of the diſtemper, they reduced to a more cautious and ſafe method.

Dr. *Cyrillus* proceeds to ſhew the rules of this method, and which were confirm'd by repeated trials: For, a water-diet, as he calls it, is a grand remedy; in adminiſtering it, as in all other grand remedies, we muſt proceed very cautiouſly, leaſt what is intended for the health of the patient ſhould prove his ruin.

The chief rule is, that the patient ſhould begin to drink upon an empty ſtomach about one or two pounds of water, cool'd with ſnow, according to his age, ſtrength, or even thirſt; the ſame quantity to be repeated every hour, or every other hour, or later, and that both day and night, without intermiſſion, unleſs he happen to fall aſleep. The patient to abſtain entirely from food: For, it is confirm'd by experience, that when the food is mix'd with a large quantity of water in the ſtomach, it not only putrifies, but likewiſe infects the ſubſtance of the water, and renders it leſs fit to paſs eaſily thro' the capillary veſſels; and conſequently, to be diffus'd thro' all the parts of the body, in order to promote the precipitation and ſecretion of the noxious humours. This abſtinence is to be continued for ſeveral days, till the fever either entirely intermit, or conſiderably remit, and the patient begin to complain of hunger: For, if food be given too ſoon, the fever, attended with all its ſymptoms, does immediately recur. Wherefore, we order our patients to abſtain from all kind of food (providing they take the water) ſometimes for ſeven, nay ten days and upwards. Nor is there any bad conſequence to be apprehended from this abſtinence; either that on account of the coldneſs of the water (the pores of the ſkin being contracted) inſenſible perſpiration, and conſequently nutrition ſhould be diminiſh'd; or that the water itſelf continually waſhing the elaboratories of the food, if they contain any remains of aliment (and they are always ſtuffed) ſhould carry them along with it, and diffuſing them thro' all the body, promote the buſineſs of nutrition as much as poſſible. Whatever be in this it is confirm'd by daily experience, that food cannot be mix'd

mix'd with large quantities of water, without very much endangering the patient: So that when he takes food, it must be given either without drinking any at all, or but very little water; nay there must be some hours of interval, that the food may be properly digested in the stomach: And the lightest and easiest of digestion is to be chosen, as panada, bread-puddings, soft eggs, and scarce any broth. These at first are to be given but once a day, and sparingly; afterwards twice a day; that the patient may gradually come to use a more plentiful dinner, yet still a sparing supper; and he must entirely abstain from meat for a month and upwards. Upon such kind of food the patient must not forbear the use of water, but after digestion be obliged to take down two or three draughts, till getting the better of the distemper, he gather strength and gradually recover.

Some things are accurately to be observ'd in the course of the cure; especially whether the water passes off easily or no: If the urine, at least a day after, begins to be voided in large quantities, and gradually become discoloured; then you may suppose the water has begun to make way for itself: Sometimes on the very first day, or on the second or third the patient purges; at first the stools are grosser, and afterwards become variegated and liquid: Hence there are greater hopes that the fever begins to remit, and the symptoms to abate, which is so infallible that if the patient have no stool for the first two or three days; even tho' the whole quantity of water should be discharged by urine; yet the passages must be lubricated by injecting glysters, and taking down oil of sweet almonds: For, the grosser humours contain'd in the first passages, and which cherish the fever; or which are commonly produced by the disease, cannot be discharged by the fine urinary passages, but must be thrown off by the large intestinal tube. Add to this, that when the first passages are cleared, the water may more easily pass into the outmost recesses of the body, and prove medicinal both to the blood, and other fluids.

Should the *parotides* swell, as they often do; or any purulent matter be observ'd to be mixt with the *foeces* or urine, which is an evident indication of an abscess, the use of the water must not be laid aside, but rather urged the more: For, the same water which could expel the humours, stagnating in the internal parts, to the surface of the body, and bring them to maturation when collected there, will likewise be able to

throw them off entirely by the several excretories, and so preserve the parts from mortification; as we frequently observe in practice. Yet we find by the same practice, that abscesses in the brain and breast are not so easily overcome by the use of water; tho' a great many have been entirely cured by discharging *pus* at the nostrils, ears, mouth, and likewise by coughing it up from the lungs. When therefore such symptoms appear, we usually have a recourse to water; but if the patient become sleepy we apply vesicatories, *&c.* or if he have a difficulty of breathing, we give the new expressed oil of sweet almonds, or lukewarm water. Besides, it frequently happens, especially on the first days of the water-diet, a violent fit of vomiting comes on; by which if heterogenous humours, as commonly happens, be thrown up, it is to be reckoned beneficial: For, the vomiting ceases, and the patient is relieved after the stomach is entirely emptied: If the water be thrown up as soon as drank, the patient must be obliged to repeat his draughts: For, malignant humours commonly ensue upon throwing up the water. In like manner if the hiccup arise, water is to be continually administred: For, we observe, that as it is caused by the use of water, so it is removed by the same. But should sweat ensue in drinking the cold water, it commonly exhausts the strength, to the great danger of the patient. This indeed, may seem a paradox to such, as expect sweat by drinking cold water in fevers, wherein we know Dr. *Hancock*'s grand febrifuge consists. But if sweat ensue in the use of the water-diet, it should be prevented, by giving the patient still colder water, and in larger quantities, and cooling his body by taking off the bed-cloaths, fanning him, and admitting the air; and some even venture to sprinkle the patient with snow.

What proves the greatest difficulty to the physician in the use of the water is, when the patient becomes either delirious, lethargic, or greatly weakened, is not able to drink the water in due quantity, or often. Then all possible pains are to be taken, to exhibit this as the last remedy, which is sometimes done by threatning the delirious, and forcibly thrusting snow into the mouths of such as are very weak and sleepy.

After having shewn the method of a water-diet, and how to obviate the symptoms, it remains to shew in what kind of fevers, and at what time of the fever, we are to have recourse to this remedy. It is not proper to use the water immediately

in the beginning of fevers: For, as all things are crude and groſs at that time, it cannot reaſonably be expected that the peccant humours ſhould be ſecreted or thrown off by a large quantity of water: But it is otherwiſe in the ſtate of the diſtemper, when, according to *Hippocrates* ἡ νοσος ἀνθέει, *i. e. the diſtemper is at its height*, namely, when the patient is in greateſt danger, then the uſe of water is ſeaſonable. For, at that time the matter that feeds the fever having acquired, by a continual ebullition, a kind of comminution of its particles, or rather ſome degree of coction, it may, by giving water plentifully, and by its admixture with the humours of the body, be precipitated, as it were, ſecreted, and thrown off wherever it can have any vent. Hence therefore, we often obſerve, that ſuch as in the very agonies of death have had recourſe to this laſt remedy, as the ſheet-anchor, have recover'd: And ſome phyſicians neglecting this caution, and adminiſtering water in the beginning or increaſe of the fever, have very much endangered their patients: Yet Dr. *Cyrillus* does not deny, but that water is ſometimes given with ſucceſs in the very beginning of bilious fevers, or ſuch as are caus'd by thin humours, and the reaſon is very plain from what has been ſaid above.

In all acute, malignant, and mortal fevers, recourſe muſt be had to a water-diet, whenever it ſhall ſeem proper; which is uſually indicated by the great danger the patient is in: For, fevers are either ſuch, as that the blood tends to concretion; and then it may again eaſily recover its priſtine fluidity by a plentiful admixture of water. Or ſuch as that the maſs of blood is apt to be attenuated too much, and the ſpirits diſſipated; and in this caſe, water, eſpecially cold, will diſſolve and break the points of the acid ſalts, which attenuate the blood. Whence ſome light is thrown upon this phenomenon, namely, why the body of the feveriſh patient, now chilled by drinking the cold water is, as it were, miraculouſly heated: For, whether the blood begins to be concreted by a fatal diſſipation of the ſpirits, or the ſpirits evaporate by too great a ſolution of the blood; a large quantity of cold water mixing with it, (from which there ariſes a conſtriction of the pores of the ſkin) will always hinder a diſſipation of the ſpirits; and conſequently reſtore the loſt heat.

Hence, Dr. *Cyrillus* would here inculcate what he hinted at firſt, namely, that water only, cooled with ſnow is to be preſcribed in fevers: For, the patient takes down at

first the necessary quantity of cold water, but commonly refuses what is warm in once or twice drinking it: Besides, from the coldness of the water the fibres of the stomach acquire a greater elasticity, by which it may be easily propell'd into the more remote passages, and into the blood itself: Whereas warm water, weakening the tone of the fibres, blows up the stomach, oppresses and cloys it.

Yet the Dr. acknowledges, that he sometimes prescribes warm, and not cold water; namely, when inflammations and pains in the lungs and bowels are complicated with fevers; least the cold water should occasion a mortification in the inflamed parts; but he does not deny, that in these cases he sometimes has recourse to water somewhat cold, tho' not cooled with snow; since patients nauseate warm water, and refuse to drink it: For, he thinks it better to give a large quantity of water somewhat cold, than a small and unavailing quantity of warm water.

According to these rules (confirmed by reason and repeated experiments) a water diet in fevers has been observ'd successful; so that we could verify this expression of *Pindar* ἀριστον μεν ὑδωρ, *water to be the best medicine*: tho' sometimes not without disappointments, a thing not uncommon in physick, especially in administering the grand remedies: For, there is nothing so infallible in that art, but it may often deceive the most accurate practitioners.

Hitherto Dr. *Cyrillus* has treated of the use of cold water in fevers only: And yet there are other distempers in which (encouraged by a certain kind of analogy) he tried a water-diet, and that to the benefit of the patient; as in *diarrhoea*'s, dysenteries, nay and in *Lienteries*; in *Ischaria*'s and *Dysuria*'s, in *Cardialgia*'s and *Cholera Morbus*, and in hypochondriac and hysteric disorders: Nor are we without instances of dropsies being cur'd by plentifully drinking water; for, it opens the fine passages of the kidneys and *Abdomen*, from whence plentifully flow both the water and the serous humour. Nay, he found the use of a water-diet beneficial in the small-pox; but that was in their third stage, when the patients were hastning to their end, by abscesses, form'd in the brain and breast; but by administering this remedy he observ'd a large quantity of *pus*, discharged by the nose and mouth: Yet the Dr. does not prescribe a water diet (that is water alone, without any food) in all these disorders; but in chronic distempers he commonly orders one large draught of cold

cold water to be taken four hours before a ſlender dinner, and another eight hours after it.

This the Dr. takes to be ſufficient for his preſent purpoſe; but there is one thing (as to the quantity of cold water to be given) that ſhould be carefully obſerved; and that is, that the error is greater in drinking too little, than too much: For, ſince this is the end of giving water in fevers to patients in a very dangerous ſtate, namely, that being diffuſed thro' the whole body, it may open the paſſages, communicate motion and their natural tenuity to the fluids, and cheriſh the heat and ſtrength of all the parts; it would be in vain to expect ſuch effects, by giving a ſmall quantity of water; which, ſtaying in the ſtomach and firſt paſſages would be ſo far from proceeding any farther, that mixing with the noxious humours, ſtagnating there, it would promote their putrefaction, and prove fatal to the patient. It muſt therefore be given in large and repeated doſes, that it may by that means make way for itſelf, and water the whole body throughout: And the uſe of it to be more boldly proſecuted, if after the firſt day, ſigns of its paſſing appear in the urine or *fœces*: For, that water may be given with greater ſafety than any other medicine appears from this; namely, that tho' ſome medicines might, probably, overcome the greateſt diſorders, when taken in a larger doſe; as *antimon. diaphoret. bezoart.* and any volatile ſalt; nay, even generous wine drank plentifully: Yet it is evident, that weak patients, eſpecially, are in very great danger, even by the exceſſive doſe itſelf: Whereas water alone ſeems to be the moſt innocent and ſafe: For, there is ſcarce any patient ſo weak, but he may bear the drinking it in a very large quantity. Hence the Dr. thinks it would not be immethodical, if, when matters are brought to the loweſt paſs, the phyſician ſhould, at leaſt, permit if not preſcribe a water-diet, tho' contrary to the indications: For we ſometimes obſerve that patients, whoſe lives have been deſpaired of, have unexpectedly recovered: So that it is to be conſidered as a ſound advice; that a doubtful remedy is better than none.

This is what has been experienced at *Naples* on the uſe of cold water in fevers and other diſtempers; ſo that it is now become an univerſal remedy in all diſorders.

Whether any ſuch thing may be attempted with ſucceſs in colder countries he leaves the phyſicians of theſe parts to determine; tho' he would not deſpair of ſucceſs, ſince cures are happily made at *Naples* even in the winter.

Of

Of the different kinds of Ipecacuanha, *by Dr.* James Douglaſs. Phil. Tranſ. N°. 410. p. 152.

THE firſt general diviſion of theſe roots may be into true and falſe; and each of theſe may be again ſubdivided into ſeveral ſpecies, the diſtinguiſhing mark of which is principally taken from the colour.

Of the true *Ipecacuanha* the Dr. had four kinds; black, brown, grey, and white; but he cannot pretend to determine, whether they belong to different plants, or are only varieties of the ſame plant, owing to the ſoil in which they grow, as is affirm'd by Sir *Hans Sloane*: And as theſe roots are never imported to us entire, it is impoſſible to give any certain deſcription of them in that ſtate.

However by comparing the ſeveral dried pieces, as we have them, we may very probably conjecture, that a ſhort radical trunk deſcends from a *caulis*; and is afterwards divided into ſeveral large branches; and theſe again into ſmaller ones, in different ſeries, with minute filaments or *fibrillæ*, going out from them.

Each piece is compos'd of an outer or cortical part, and an inner or fibrous one, which, like a white nerve, or ſmooth compact *faſciculus* of woody filaments, run's thro' the center or axis of the roots, and perhaps encloſes within it a ſmall pith, which, however, is hardly diſcernible by the naked eye.

The cortical part has two ſorts of *rugæ* or wrinkles; one ſuperficial, conſiſting either of circular rings or little knots, which do not go quite round; the other penetrating into its ſubſtance, being deep inciſures or fiſſures, reaching all the way to the nerve.

What lengh theſe roots are of, when taken out of the ground, cannot be determined: The Dr. met with ſome pieces upwards of 9 inches; ſeveral upwards of 6; but the greateſt part are ſtill ſhorter.

We find them bent, wreath'd and contorted into all manner of figures; and indeed, few pieces are altogether ſtreight for any conſiderable length.

What has been hitherto ſaid, agrees to all the true *Ipecacuanha* roots; but ſeveral other particulars are ſtill to be taken notice of, in which they differ.

The black ſort is the ſmalleſt of the 4; very hard and the fiſſures wide and numerous. The outer colour of the *cortex* is not equally black in all the pieces of this kind; and its inner

ner ſubſtance, as well as the nerve, is moſtly white; tho' not always in the ſame degree.

The brown ſort is larger than the black; the fiſſures at larger diſtances; the inner ſubſtance of the *cortex* darker, and the external colour has ſeveral degrees of red in the ſeveral pieces.

The third or grey ſort is ſometimes found of a darker, ſometimes of a lighter colour; and the inner ſubſtance of the *cortex* is brown, ſtreak'd with white: It is much larger than the black ſort; ſeveral pieces being upwards of $\frac{1}{4}$ of an inch in diameter; but the nerve is ſmaller in proportion to the cortical part. The Dr. met with ſome pieces of this ſpecies above five inches in length: But as he already obſerved, nothing can be concluded from thence, as to the length of the entire roots. The fiſſures are here ſtill fewer than in the brown ſort; and in ſome pieces ſcarce any are to be met with. The ſuperficial corrugations are various in different roots; ſome being almoſt entirely ſmooth; and in others the *rugæ* are rather longitudinal than circular.

The white kind, as far as the Dr. could judge by the ſmall ſample he had of it, is of different ſizes; ſome pieces of it being larger than any of the grey ſort, and the reſt much leſs. The whitiſh colour of the *cortex* has a yellowiſh caſt, and the nervous part is very large in proportion to the reſt.

Very few fiſſures are to be obſerved therein, and hardly any reach as deep as the nerve. The other *rugæ* are likewiſe very ſhallow, and moſt of them longitudinal; but it ſeems to be more knotty than the other kinds; and theſe knots he takes to be chiefly owing to the fibrils, which go out from the larger branches of the roots.

The places where theſe different ſpecies of *Ipecacuanha* grow has not hitherto been fully determined.

The black ſort is hitherto known to come only from *Braſil*, by the way of *Lisbon*; and ſome of our druggiſts for that reaſon diſtinguiſh it by the name of the *Braſil* root.

As to the brown ſort, the Dr. was informed by Dr. *James Houſton*, who reſided for ſeveral years in *New Spain*, that it grows in plenty at ſome diſtance from the city of *Cartagena* in the kingdom of *New Granada*; from whence it is frequently ſent in *ſaroons* or ſkins, containing 100 weight, to *Jamaica* and ſo to *England*; where it is certain we have had it of late years in great abundance.

The

The grey *Ipecacuanha* is with us preferred to all the reſt, and by far the moſt generally uſed, when it can be had. It is ſaid by Authors to grow in *Peru*, from whence it is brought to *Porto-Bello*; and from thence into *Europe* by the *Spaniſh Galleons*. Some parcels of it are likewiſe, probably ſent from *Porto-Bello* to *Jamaica*: For, we are certain, that it has ſometimes been imported hither from that iſland: By ſome ſpecimens that were brought the Dr. from St. *Thomé*, a *Portugueſe* iſland under the equinoctial, whither they were ſent directly from *Braſil*; it is evident that this ſpecies is likewiſe a native of that country; and therefore muſt either have been included by *Piſo* under one of the two ſpecies, mentioned by him, or elſe diſcovered ſince his time. According to F. *La Bat*, in his late voyage to the iſlands of *America*, this ſpecies likewiſe grows plentifully in *Martinico*, where for many years paſt it has been uſed by the inhabitants.

The white ſort, called by the *Portugueſe Ipecacuanha blanca*, is ſaid by *Piſo* to grow in *Braſil*; and if we believe F. *La Bat*, it is likewiſe found in *Martinico*.

Theſe are the four kinds of true *Ipecacuanha* which have hitherto come to the Dr's knowledge: But he has met with two other roots, to which the name has been falſely aſcribed; which from their external colour he calls white, and reddiſh brown.

The white ſort agrees pretty much both in colour and ſurface with the true white, but it is not near ſo knotty: It is likewiſe conſiderably larger in ſize, ſtreighter and ſofter to the touch.

The brown ſort is of a deeper colour than the true brown; and ſeveral pieces of it have ſome mixture of red (whence it has ſometimes been called red *Ipecacuanha*) and the inner ſubſtance of the *cortex* inclines to a reddiſh yellow. The pieces of it are much longer than any of the former ſorts; ſome of them meaſuring 16 inches: and they are of a ſize between the black and grey. The fiſſures are at greater diſtances from one another than in the true brown, and the ſpaces between them much ſmoother: In a word, tho' this root, when mixt with the true brown, to which it bears the greateſt reſemblance, may eaſily be confounded therewith; yet when they are attentively compared, their whole appearance ſufficiently diſtinguiſhes them.

Both theſe falſe kinds were brought the Dr. from *Maryland* in 1725, by a ſurgeon, who informed him that they grow there in great plenty, being called *Ipecacuanha* by the inhabitants, and uſed as a vomit by thoſe of inferior rank. Since that time the Dr.

Dr. received a ſample of the brown ſort, taken from a parcel, which lay in the *Cuſtom-houſe* upwards of 12 years, and called wild *Ipecacuanha*.

Sir *Hans Sloane* informed the Dr. that this falſe brown ſort was the ſame that was formerly ſent him from *Virginia* for the true *Ipecacuanha*; and which he afterwards diſcovered to be the root of a poiſonous *apocynum*, deſcribed by him in his *Natural Hiſtory* of *Jamaica*, in which iſland it is very common, and likewiſe in *New Spain*, as appeared by the ſpecimens ſent him by Dr. *Burnet*.

An Account of ſome new aſtronomical Diſcoveries relating to the Planet Venus; *by S.* Bianchini. Phil. Tranſ. N° 410. p. 158.

THESE diſcoveries S. *Bianchini* diſpoſes under four heads.

1. The deſcription of the duſky ſpots obſerved in her diſk.

2. Her rotation round an axis, the poſition of which is determined by the apparent motion of thoſe ſpots, together with the time of her revolution.

3. The paralleliſm of that axis to itſelf in all parts of the planets orbit.

4. Obſervations in order to determine the horizontal parallax of *Venus*; and conſequently, thoſe of the ſun and other planets.

He takes notice of five remarkable ſpots in her whole ſurface; the two ſmalleſt of which are placed, one near each pole; the other three lie along the equator, and cover good part of a zone, extended to about 30 degrees of Lat. on each ſide. He repreſents them to be much like the larger dark ſpots in the moon, which are uſually called ſeas, but conſiderably fainter; ſo as not to be eaſily diſcernible even to a ſharp-ſighted obſerver, without the aſſiſtance of a teleſcope, capable of repreſenting the planet diſtinctly under an angle, equal at leaſt to that under which the moon appears to the naked eye, and with an aperture of 3 or 4 inches of a *Roman* palm. He then proceeds to give the deſcription of a machine, contrived by him to repreſent to the ſight the motion of the earth and *Venus* in their orbits; and by means of a lamp placed in the centre, to ſhew the phaſes of the planet, and appearance of the curve lines, deſcribed by the revolutions of the ſpots round the axis.

This revolution he makes widely to differ from thoſe of the earth and *Mars* (the two bodies next in order in the planetary

ſyſtem) both in the poſition of the axis and time of the period: He places the ſolſtitial colure, or plane paſſing thro' the axis of the planet, and tropical points of its orbit, about the 20 degree of *Leo* and *Aquarius*, and gives the planes of its equator and ecliptic an inclination to each other of about 75 degrees. He determines the time of the revolution to be about 24 days and 8 hours, inſtead of 23 hours, as it has generally been taken to be from ſome obſervations, made by M. *Caſſini* in 1666 and 1667; but which he himſelf did not ſeem much to rely on. Now, both theſe periods may be very conſiſtent with the ſame obſervations, provided that one of the obſervers did not continue his obſervations for any conſiderable time at once: For, if the exact ſituation of any ſpot be obſerved at any given hour one day, and at the ſame hour the ſucceeding day be found advanced about 15 degrees or $\frac{1}{24}$ of the whole revolution, it may ſtill remain doubtful, whether the ſpot has moved only thro' thoſe 15 degrees in that day, or has made one or more entire revolutions beſides, in that time. This the author was aware of, and therefore, waited for an opportunity of attending to the motion of a ſpot as long at once, as the nearneſs of *Venus* to the ſun would admit of.

Accordingly *Feb.* 26. 1726, a little after ſun-ſet, he obſerved a ſpot near the center of her diſk, where its motion is moſt perceptible in a ſhort time; and about three hours after, perceiv'd the ſame ſpot not ſenſibly removed: From which he concluded, the period of its revolution could not be ſo ſhort as one day; ſince if it were ſo, the change of place of the ſpot muſt have been very ſenſible in that time. It were to be wiſh'd the Author had had opportunities of confirming this period by more obſervations; eſpecially ſince it was neceſſary to begin them ſoon after ſun-ſet, and continue them till *Venus* were near the horizon; the ſtrength of the twilight in the firſt caſe, and the thickneſs of the atmoſphere thro' which the planet muſt be obſerved in the latter, rendering the obſervations very difficult.

The next article of *Bianchini*'s obſervations is the contrivance of the axis in the ſame paralleliſm, thro' the whole orbit of the planet. This is ſo neceſſary and obvious a conſequence of the eſtabliſhed laws of motion, that there needs no more be ſaid about it.

The obſervations to determine the parallax of *Venus* were made in 1716. The method he uſed for this purpoſe was to take the ſeveral diſtances of time, between the appulſe of the limb of *Venus* and of *Regulus* (which ſtar ſhe paſſed about

that

that time) to a horary circle very near the meridian, and to another about six hours after, which he measured by the pulses of a watch, 143 of which were equal to one first minute of time. He likewise observed the alteration of those distances, taken at the same hour several days, one after another; and allowing a proportional alteration for the time between the two observations, he computed what the difference of their right ascension ought to have been in the latter of them, if there were no parallax; then comparing this difference with that observed, he concluded the disagreement to be the parallax of right ascension. This method the Author seems to depend on so much, as to think that an equal degree of exactness is hardly to be expected from any other hitherto practised: But if we consider that the whole parallax of right ascension amounts by his observations to no more than four pulses of his watch; and that he allows a possibility of an error near one of those pulses in taking each of the transits; it is evident that if such an error be actually committed in each of the observations, on which the finding of the parallax depends; and all of them happen to conspire the same way, the result of all together may possibly be greater than the whole parallax found. Upon the whole, he makes the horizontal parallax of *Venus* at that time to have been $24'' 20'''$, and that of the sun $14'' 18'''$; but as he takes no notice of the latitude of the place, in deducing the horizontal parallax from that of right ascension, they both ought to be increased on that account by about $\frac{1}{3}$, or in the ratio of 3 to 4. If therefore, there be no other mistake in his numbers, the horizontal parallax of the sun, as deduced from his observations, should be about $19''$.

For a telescope of 100 *Roman* palms he allows an aperture of 3 or 4 inches of that palm, with an eye-glass whose focal length may be from 7 to 11 inches of the same palm; but what he directs in longer instruments to increase the breadth of the aperture, and focal length of the eye-glass, in the same proportion with the instrument, must certainly be the effect of some mistake. For, in this case, a longer telescope will magnify no more than the shorter; but only have the strength of light in the object, increased in proportion to the square of the length.

Observations on a Treatise Writ by M. Helvetius *of* Paris, *to prove that the* Lungs *do not divide and expand the* Blood; *but on the contrary cool and condense it; by Dr.* Nicholls. Phil. Transf. N° 410. p. 163.

THE matter in question between M. *Helvetius* and S. *Michelotti* is, whether the lungs cool and condense the blood, according to the opinion of the ancients; or whether they mix, attenuate; and consequently expand it, according to Dr. *Pitcairn*'s system.

The Author in order to support the opinion of the ancients, brings several arguments to confute Dr. *Pitcairn*'s system: The most considerable of which, (and which indeed he makes his *argumentum crucis*) is, that the right auricle and right ventricle being considerably larger than the left, and the pulmonary artery having a larger capacity than all the pulmonary veins, taken together; the blood must evidently possess a greater space before, than after its passage thro' the lungs; and because the difference in the capacity of these vessels cannot be balanced by any increase of the velocity, he concludes, that the blood is not attenuated and expanded, but must be condensed in its passage thro' the lungs. And this he conceives is done by the air, which (as a fluid relatively cold) must cool and condense the blood, to which it is so nearly applied in the action of inspiration.

That the blood is cool'd by the action of inspiration is a matter, which Dr. *Nicholls* believes few doubt of, when they consider that in inflammations of the lungs, nothing is more earnestly desired than the breathing cool and fresh air; nor does any thing more evidently conduce to the cure of inflammatory indispositions, than the use of fresh air.

If we consider the state of the blood at its return to the heart, and how careful nature has been, not to use this blood for the nourishment of the lungs, before it has past thro' the pulmonary vein and artery (tho' it would in that case have been as effectually cool'd in the bronchial arteries, as in the pulmonary vessels) we are naturally led to believe, that it is some other quality, which has rendered it improper for nourishment, and which is to be destroyed by the action of the lungs.

For this reason, and from the structure of the parts subservient to breathing, it seems evident, that the blood is mixed, attenuated; and consequently re-expanded in the action of expiration. At present Dr. *Nicholls* considers, whether the action of

of inſpiration ſo far over-balances that of expiration, as to condenſe the blood into a leſs bulk, than it had before its paſſage thro' the lungs.

The accurate S. *Santorini* of *Venice*, in chap. 8. ſect. 3 of his obſervations, has carefully examined the fact, as ſtated by *Helvetius*; and finding it true in that one ſubject, as to the auricles, and pulmonary veſſels, but falſe as to the ventricles; he proceeds to prove, that this difference in the capacity of the pulmonary veſſels, could not be deſign'd on account of the blood's being condens'd in its paſſage thro' the lungs; becauſe if ſo, the right ventricle ought to have been larger than the left; and the pulmonary artery ought, not only to have been larger than the pulmonary veins, but likewiſe to have been larger than, or at leaſt equal to the two *venæ cavæ*; whereas in his ſubject the two *cavæ* were to the pulmonary artery, as 228 to 188.

In the mean time he recommends to other anatomiſts, the repeating the enquiry, as doubting whether the fact be conſtantly ſo in healthy ſubjects.

As ſuch an enquiry may be of conſequence, not only in ſettling the point in queſtion, but in explaining other parts of the animal œconomy; Dr. *Nicholls* doubts not but it will be acceptable; and the rather, becauſe the ſubjects, from which he has taken the ſeveral calculations, were produced before the Society, and ſubmitted to a re-examination if deſir'd.

The meaſure the Dr. has here made uſe of is the 113 part of an inch.

He has taken the triple of the diameter for the periphery, and computed the area, by multiplying the neareſt whole number to a fourth of the diameter into the periphery. Tho' this method be not ſufficiently exact to ſhew the real contents of circles; yet as the Dr's deſign here was only to find nearly the relative contents of the ſeveral veſſels; he has choſen to avoid embarraſſing the ſums with fractions.

The firſt heart is that of an adult, in which

	The diam.	Perip.	and Area's	
Of the deſcending *Cava*	79	237	4740	
Pulmonary artery	115	345	10005	
Superior left pulm. vein	69	207	3519	12477
Inferior left pulm. vein	73	219	3942	
Superior right pulm vein	49	147	1764	
Middle right pulm. vein	40	120	1200	
Inferior right pulm. vein	57	171	2052	

Arta

Aorta	110	330	8910

The aſcending *Cava* being tied above the diaphragm, could not be meaſur'd in this ſubject.

As M. *Helvetius* does no wiſe mention the diſeaſe, of which the ſubject died, from which he took his obſervation; ſo the Dr. cannot ſay how proper it was for ſuch an examination: But it is evident, his obſervation does not tally with the calculations made from this firſt heart; where the pulmonary artery is to the ſum of all the pulmonary veins as 10005 to 12477. And yet this ſubject (beſides a cancerated *ovarium*, and a putrefaction of the right kidney from the *ureters* being compreſs'd) had her lungs full of ſmall tubercles; and the glands, lying between the large diviſions of the *trachea*, almoſt petrefied by atheromatous concretions: By all which it is highly probable, that the paſſage of the blood thro' the lungs, was very much impeded; and conſequently the pulmonary artery much dilated beyond its natural capacity.

And this the Dr. is the rather induced to believe from examining the ſecond heart, which is that of a child near 12 months old. As to its death, he can ſay nothing more, than that its lungs appear'd perfectly ſound, and of a pale clear colour, and therefore more proper for an examination of this kind.

In this ſecond heart the	Diam.	Per. and	Area's	
Of the *Aorta* above the coronaries	43	129	1419	
Pulmonary artery	43	129	1419	
Superior left pulmonary vein	29	87	609	2088
Inferior left pulmonary vein				
Superior right pulmonary vein	26	78	507	
Middle right pulmonary vein	17	51	204	
Inferior right pulmonary vein	32	96	768	

We may here obſerve that the *Aorta*, after ſending out the coronary veſſels, is equal to the pulmonary artery. As to the proportion between the pulmonary artery and veins; the artery in this ſubject is to the ſum of all the veins here meaſured, as 1419 to 2088; and yet the lower left pulmonary vein is here omitted, as being tied too cloſe to admit of being meaſured. But if we ſuppoſe the inferior left pulmonary vein to be to the ſuperior left pulmonary vein, in the ſame proportion as in the firſt

first heart, we ſhall then find its diameter nearly 31, and its area at leaſt 700, which will make the pulmonary artery in this heart to the ſum of all the pulmonary veins, as 1419 to 2788; and in that caſe, the left pulmonary veins will be to the right pulmonary veins, but as 1309 to 1479.

The third heart is that of an abortive, nearly of five months: By its appearance the Dr. judged it had been ſuffocated by too much blood. In this ſubject.

	Diam	Per.	Areas.	
Of the deſcending *Cava*	14	42	197	629
Aſcending *Cava*	24	72	432	
Aorta above the coronaries	16	48	192	
Pulmonary artery	20	60	300	
Canalis artereoſus	12	36	108	
Right pulmonary branch	11	33	99	198
Left pulmonary branch	11	33	99	
Superior left pulmonary vein	11	33	99	294
Inferior left pulmonary vein	9	27	54	
Superior right pulmonary vein	7	21	42	
Middle right pulmonary vein.	11	33	99	

The inferior right pulmonary vein is here cut too cloſe, and otherwiſe injur'd; ſo that its area cannot be meaſured. Nevertheleſs we find the remaining pulmonary veins to the pulmonary branches of the pulmonary artery, as 294 to 298.

We may here obſerve a remarkable difference between the capacities of the two *venæ cavæ* taken together, and the pulmonary artery; both the *cavæ* being more than double the pulmonary artery; and the pulmonary artery ſtill a third larger than the *Aorta*. As this difference could not ariſe in this caſe from the blood's being condens'd by the inſpir'd air; ſo it ſeems to prove, that had the fact been true, as ſtated by *Helvetius*, it had nevertheleſs been an inſufficient demonſtration of his ſyſtem.

A Lunar Eclipſe obſerv'd at Rome, Feb. 2, 1728-9, *by* F. Carbone, Phil. Tranſ. N° 410. p. 170. *Tranſlated from the* Latin.

True time			Immerſions.
H.	M.	S.	
	44	22	The beginning of the Eclipſe.
5	46	16	*Grimaldus* immerges.

Keplerus

	48	8	*Keplerus* immerges.	
	54	20	The beginning	
	54	46	The middle	of *Copernicus*.
	55	10	The end	
8	11	57	The beginning	
	13	7	The middle	of *Tycho*.
	13	48	The end	
	19	0	*Manilius* immerges.	
	20	50	*Menelaus*	
	23	0	*Dionysius*	
	29	44	*Plinius*	
	31	6	The middle	of *Mare Tranquilitatis*.
	33	1	The end	
	34	41	The beginning	of *Proclus*.
	35	29	The end	
	36	1	*Mare Crisium* begins to immerge.	
	39	44	Entirely immerged.	
	43	17	The Moon entirely immerged.	

Emersions.

10	21	38	The first limb of the moon,	
	23	27	*Ricciolus*	
	24	7	*Grimaldus* begins to emerge.	
	25	4	Entirely emerged.	
10	34	39	*Aristarchus* begins to emerge.	
	36	8	Entirely emerged.	
	41	11	*Tycho* begins to emerge.	
	42	5	Entirely emerged.	
	47	10	*Helicon* begins to emerge.	
	48	14	Entirely emerged.	
	54	33	*Plato* begins to emerge.	
	54	57	Entirely emerged.	
	57	54	*Aristoteles* emerged.	
11	2	5	*Menelaus*.	
	4	33	The middle	of *Mare serenitatis*,
	9	15	The end	
	14	36	*Possidonius* emerged.	
	16	7	*Cleomedes*.	
	16	20	The half of *Mare Crisium* emerged.	
	17	36	Entirely emerged.	

the

The end of the Eclipses 11^h 20′ 41″. The ſame day the meridian diſtance of the Sun's center (not corrected by refraction) was obſerv'd to be 55° 9′ 31″ in a *gnomon*; whoſe meridian a ſolar eclipſe, projected on the pavement, paſſed over in 2′ 15″; and the apparent diameter of the ſun intercepted 2945 parts of the micrometer, of which the moon, obſerved a little before the eclipſe, intercepted 2903. The obſervations were made with a teleſcope of eight *Roman* feet and a quarter.

The ſame Eclipſe obſerved at Paris. Phil. Tranſ. N°. 410 p. 171. *Tranſlated from the* Latin.

Time H.	M.	S.	Phaſes.
7	1	0	A denſe *penumbra.*
7	3	0	A very denſe *penumbra.*
7	3	0	The begin. of the eclipſe deduc. from other phaſes
7	8	50	*Galilæus* cover'd.
	14	0	The ſhadow at *Ariſtarchus.*
	15	4	*Ariſtarchus* entirely immerged.
	16	44	*Keplerus* entirely immerged.
	18	4	The ſhadow at *Gaſſendus.*
	19	20	*Schickardus* entirely cover'd.
	22	0	The ſhadow at *Reinholdus.*
	22	40	At the limb of *Copernicus.*
	23	43	*Eratoſthenes* cover'd.
	25	15	*Copernicus* entirely immerged.
	27	2	*Helicon* entirely immerged.
	31	50	The ſhadow at *Tycho's* limb.
	33	8	The half of *Tycho* immerged.
	33	30	The ſhadow at the preceeding limb of *Plato.*
	33	47	*Plato* entirely immerged.
	38	7	The ſhadow at the preceeding limb of *Manilius.*
	39	20	*Manilius* entirely immerged.
	41	45	The ſhadow comes to *Menelaus.*
	42	35	The ſhadow at the middle of *Menelaus.*
	45	22	The ſhadow at *Plinius.*
	49	47	At the preceeding limb of *Fracaſtorius.*
	50	30	At *promontorium acutum.*
	51	24	The ſhadow covers *Fracaſtorius.*
	54	30	It touches *Proclus.*
	55	16	*Proclus* entirely cover'd.
	56	17	The ſhadow at the limb of *Mare Caſpium.*
	58	56	At the half of *Mare Caſpium.*
	59	0	At the following limb of *Mare Caſpium.*
8	2	0	The end doubtful.
	3	0	The end certain.

Time. H.	′	″	Emersions.
9	41	18	The beginning of the emersion.
	41	33	*Grimaldus* begins to emerge.
	45	40	*Grimaldus* entirely emerged.
	49	35	*Galilæus*
	51	30	*Schickardus.*
	54	34	*Capuanus.*
	55	16	*Aristarchus* begins to emerge.
	56	5	Entirely emerged.
	58	35	*Keplerus.*
10	0	30	The first limb of *Tycho* emerged.
	1	30	The half of *Tycho* emerged.
	2	30	Entirely emerged.
	3	40	*Lansbergius* and *Reinholdus.*
	5	19	*Copernicus* begins to emerge.
	6	43	*Copernicus* entirely emerged.
	7	33	*Eratosthenes* emerges.
	8	0	*Helicon* entirely emerged.
	12	56	*Plato* begins to emerge.
	14	15	Entirely emerged.
	20	35	*Manilius* begins to emerge.
	21	28	Entirely emerged.
	23	50	*Menelaus* entirely emerged.
	27	25	*Plinius.*
	30	19	*Dionysius.*
	31	0	*Promontorium acutum.*
	36	15	*Proclus.*
	37	26	*Mare Caspium* begins to emerge.
	41	24	The end, doubtful.
	42	0	The end, certain.

The same Eclipse observ'd at Padua *by* S. Polenus. Phil. Trans. N°. 410. p. 173. *Translated from the* Latin.

App. Time. P. M. H.	′	″	The phases were observ'd with a very good telescope 7 *Paris* feet in length.
7	44	40	Thick clouds hinder'd the observing the beginning of the eclipse.
7	45	40	The shadow touches *Grimaldus.*
7	50	53	It entirely covers *Grimaldus.*
7	53	26	It touches *Mare humorum.*
8	19	34	It covers the half of *Mare humorum,*
8	38	10	It entirely covers *Mare Crisium.*
9	26	0	The moon was observ'd very ruddy between the opening of the clouds, and so distinctly, that

S. *Pole*

			S. Polenus does not remember that ever she appear'd so clear in a total immersion; and this appearance, probably was owing to the darkness the circumambient clouds formed about her.
10	15	6	The shadow began to be dilute over against that part that was next to emerge.
10	26	45	*Grimaldus*, now emerged, is distant from the shadow almost its entire transverse diameter.
10	31	40	The half of *Mare humorum* emerged.
10	38	45	*Tycho* entirely emerged.
10	50	12	*Eratosthenes* emerged.
11	13	27	*Promontorium somni* entirely emerged.
11	19	45	The moon seems to be tinged with the *penumbra* only.
11	20	56	The end of the *penumbra*.

An Eclipse of the Moon July 19. 1729, *at* Wirtemberg; *by M.* Weidler. Phil. Trans. N° 410. p. 174. *Translated from the* Latin.

Time. H.	M.	S.	Phases
0	1	30	The beginning under *Grimaldus* in the morning *August* 9. N. S.
0	3	45	*Galilæus.*
0	6	0	The shadow touches *Aristarchus*
0	11	0	*Keplerus.*
			After this clouds cover the moon.
0	54	0	The half of *Mare Crisium* cover'd.
0	57	0	Entirely cover'd.
1	1	0	The total immersion.
2	40	30	The emersion.
2	43	30	*Galilæus*
2	45	0	*Aristarchus*
2	49	0	*Keplerus*
2	54	45	*Copernicus* begin to emerge.
2	55	30	*Plato*
3	1	30	*Timocharis*
3	4	0	*Manilius*
3	8	30	*Tycho* entirely emerged.
3	10	30	*Menelaus*
3	13	0	*Plinius* begin to emerge.
3	18	0	*Cleomedes*
3	23	30	*Mare Crisium* together with *Mare Nectaris* begin to emerge.
3	29	0	*Mare Nectaris* entirely emerged.

Time			Phases.
H.	M.	S.	
3	31	30	*Mare Crisium.*
3	34	50	*Langrenus* begins to emerge.
3	40	0	The end.
			The shadow going off between *Langrenus* and *Petavius.*

The same Eclipse observ'd at Padua *by S.* Polenus. Phil. Trans. N° 110. p. 176. *Translated from the* Latin.

Apparent time			Phases
H.	M.	S.	
0	0	28	The beginning of the shadow at the moon's limb.
	13	55	The shadow touches *Copernicus.*
0	15	49	*Copernicus* entirely covered.
	22	24	It touches *Tycho*
	24	14	*Tycho* entirely covered.
	28	40	The shadow touches *Manilius.*
	30	15	*Manilius* entirely covered.
	33	2	The shadow touches *Menelaus.*
	34	22	*Menelaus* entirely covered.
	49	10	The shadow touches *Mare Crisium.*
	54	56	*Mare Crisium* entirely covered.
	58	48	The total immersion.
2	37	38	The light at the moon's edge.
	41	20	*Grimaldus* emerged.
3	4	15	*Mare serenitatis* begins to emerge.
	6	16	*Tycho* entirely emerged.
	7	28	*Manilius* entirely emerged.
	10	30	*Menelaus.*
	13	58	*Mare serenitalis.*
	21	48	*Promontorium somni,*
	23	10	*Mare Crisium* begins to emerge.
	25	28	*Mare Nectaris* and the half of *Mare Crisium* emerged.
	29	0	*Mare Crisium* entirely emerged.
	33	20	*Langrenus* emerged.
	38	8	The end of the emersion, even out of all the *penumbra.*

A Geographical Description of the Kingdom of Tunis; *by Mr.* Thomas Shaw. Phil. Trans. N° 411. p. 177.

FROM *Tunis* Mr. *Shaw* travell'd as far westward as *Hydra*; and from thence he went to *Toser*, passing from *Tegewse* thro' the lake of *Marks* (or *Palus Tritonia*, as he takes

takes it) to *Gaps*: From *Gaps* he travell'd all the way upon the coast to *Biserta*; he made use of a small, but very good mariner's compass, and found the variation at *Cairwan* 10 degrees west; at *Biserta* something more than 12 degrees; and at *Algier* 30° 30′. He likewise carried along with him a brass quadrant of a foot radius, and took the latitudes of *Tunis*, *Cairwan*, *Spetula*, *Gaffsa*, *Toser*, *Ebillee*, *Gaps*, *Stax*, *Susa*, *Lowharia*, and *Biserta*, with all the exactness such an instrument would admit of. As to the longitude, most mariners whom Mr. *Shaw* convers'd with, agree within 10 or 12 miles, that the distance between *Algier* and the *Goletta* (or part of *Tunis*) is 400 miles. He made this voyage four times; and the reckonings, made aboard, amounted only to 390. He, therefore, made the meridional distance between this place and *Cape Carthage* 350 miles (allowing 48 to one degree of longitude) for, as this whole course is not upon the same parallel, we may very well allow 40 or 50 miles for the oblique sailing; because the course is in 37° 20′ N. Lat. But *Algier* lies in 36° 48′, and the *Goletta* in 36° 40′.

The kingdom of *Tunis* is bounded to the north and east with the *Mediterranean*; to the west with the kingdom of *Algier*, and to the south with that of *Tripoli*. It is 230 miles in length from the isle of *Gerba* in Lat. 33° 24′. to *Cape Serra* in Lat. 37° 16′, and 128 miles in its greatest breadth from *Monasteer* to *Tibesa*: *Sheka* its utmost boundary to the west lies in Long. 7° 26′; and *Clybea* its utmost boundary to the east in 10° 47′ from *London*.

Of the modern Geographers, *Luyts* seems to have been the best acquainted with its extent in general, giving it 3° of Long. and upwards of 4° in Lat. The *Sansons* place it upwards of 3° farther to the south than it should be, and their error is greater in relation to the longitude. *Moll* places it a few minutes only too far to the north; but to the south he has extended it beyond the parallel of *Tripoli*, wherein Mr. *Shaw* finds he has been followed by M. *De Lisle* in his map of *Africa* in 1722. But a long chain of mountains, which run in the same parallel of Lat. with *Gerba*, are the limits of *Tunis* and *Tripoli*.

If we take the ancients for our guides, we shall still find farther errors and disagreements. For, *Ptolemy* makes the difference of Lat. between *Carthage* and *Gaps*, almost the two extremities of the kingdom, to be only 1° 50′; provided the *Italian* copy Mr. *Shaw* makes use of be correct. The like distance he puts between *Gaps* and *Toser*, thereby making the latter 110 miles more to the south; whereas Mr. *Shaw* found it 18 miles more to the north. Thus again he places

Gaffsa

Gaffsa in Lat. 29° 45′ and *Gaps* in 30° 30′, making the latter a great way to the north: Whereas the courſe from *Gaffsa* to *Gaps* is near 80 miles ſouth-eaſt: not to ſpeak of his placing *Carthage* (and ſo reſpectively of other places) too far to the ſouth by near 4° 30′ or 270 miles. The like errors may be obſerv'd, as to his difference of Long. of particular places; and as to his ſcale of Long. in general, which he places at leaſt 10° too far to the eaſt.

The *Antonine Itinerary* will alſo admit of ſeveral doubts and contradictions, as *Ricciolus* has already obſerv'd *Geogr.* p. 74, and therefore it is not altogether to be depended on: tho' it muſt ſtill be allow'dto be a much better guide than *Ptolemy*. This the author of the *Itinerary* makes it to be 216 miles from *Sufetula*, Mr. *Shaw* preſumes by the way of *Adrumettum* to *Clypea*: thereby making *Clypea* 111 miles from *Adrumettum*; whereas in another place in his *Maritime Itinerary*, he only makes a difference of about 44 miles, or 350 furlongs: And again he makes the direct road from *Carthage* through *Laribus* and *Theveſte* to *Cirta* to be 332 miles; but the road by *Hippo Regius*, or *Bona*, which ſhould be farther, only 312. So that great caution is to be obſerv'd in following that auhority.

Pliny is not ſo particular as either *Ptolemy* or the *Itinerary*. He lays down things in general; and therefore can give but little light and aſſiſtance to a traveller, in pointing out to him the ancient boundaries, or the particular cities of this kingdom. His alphabetical collection of towns has but little inſtruction in it; and where he would ſeem to follow ſome order and method, as naming the towns along the coaſt of *Byzacium*, he places *Adrumettum* and *Ruſpina* after *Leptis*; thereby inſinuating, as if *Leptis* lay at a greater diſtance from the leſſer *Syrtis*; the contrary to which is prov'd eaſily from *Hirtius* and other authors. And if with *Cluverius*, &c. we ſhould make the *Africa* of *Pliny*, comprehending even the two provinces of *Zeugitana* and *Byzacium*, to be the kingdom of *Tunis*, we ſhall meet with conſiderable difficulties in the geography, eſpecially of *Byzacium*, which is the ſouthern, and ought to be the greater part of it. For as *Pliny* makes it only 250 miles in circuit, and to extend from *Adrumettum* or *Hercla*, north to *Sabrata*, or to *Gaps* only, or *Tacape* ſouth, we ſhall find that this number of miles will not be ſufficient to meaſure the coaſt twice over; and therefore can lay no claim at all to any part of the continent. But how far ſhort ſoever this calculation may be of the truth; it ſeems very probable that the province of *Adrumettum*, as deſcribed by *Ptolemy*, how faulty ſoever he may be in particulars, is the *Byzacium*, which

which we look after; and that it included the *Blaide el Gereed*, or *Country of Dates*, which *Pliny* and the author of the *Itinerary* seem to have known nothing of, or not to have regarded. For *Ptolemy's Usulitanum, Turza, Zugara*, cities still preserving their old names, and nearly of the same latitude with *Adrumettum*, continue to remain its boundaries to the north; as *Toser* and *Gaps*, the *Tisuro* and *Capi*, or *Tacape* of the ancients, do to the south; while *Tœney* and *Gaffsa*, or the ancient *Thœney* and *Capsa* determine the midland continent. And in this situation *Strabo, Geogr. lib.* 2. seems to place his *Byzacii, supra syrtes Psyllos atque Nasamones atque Getularum aliquos: Deinde sintas & Byzacios usque ad Carthaginiensem regionem: Ea enim est multa:* And at the same time he makes the country of the *Carthaginians* to be only the *Zeugitana* of *Pliny* contrary to the opinion of some geographers, who give it a much greater extent. However, *Zeutana*, or at least the greater part thereof is still call'd *Fregea*, or *Frikœa* by the *Arabs:* And as this, undoubtedly, is a corruption of its antient name, so the tradition of it through so many ages, may, perhaps, be a stronger argument, that this was the *Africa*, properly so call'd, of *Pliny*, or the province of *Africa*, by way of eminence, than most of the geographical reasons which have hitherto appeared to the contrary.

The kingdom of *Tunis* then contains the *Africa propria* of *Pliny*: with the *Byzacium* of *Strabo*; or the province of *Adrumettum* of *Ptolemy*, to which we are likewise to add so much of *Numidia* as lies half a day's journey, or six leagues west of *Keff*: For *Keff* or *Sicca Venerea* is now part of these dominions; and which both *Ptolemy* and *Pliny* place in *Numidia*, tho' it be almost in the same meridian with the river *Tusca*.

Observations on the Crane, *with Improvements; by Dr.* Desaguliers. Phil. Transf. N° 411. p. 194.

WHEN great weights are to be rais'd from a considerable depth, and laid on carriages very near the precipice, as for instance, at the edge of a stone quarry; the crane must be a fixt one; and only the gibbet moveable, from which the weight hangs. Here in the common way, the rope R *r r* or chain, (represented Fig. 3. Plate XIII.) which runs over the gibbet, goes between two pullies P Q, fixt within the upper horizontal beam of the crane A Q T X, above the axis of the gibbet B G V; so as to be easily carried to the right or left hand from W to w, when the gibbet turns upon its axis to bring the burthen over the carriage, design'd to receive it: For this purpose a small rope, call'd the guide-rope

rope, is faſtened to the weight, or to the upper part of the gibbet near its extremity *g*; which a man is to pull, in order to bring the weight over the place, to which it muſt be lowered. Now in performing this, the main rope or chain, not continuing parallel to the arm of the gibbet, gives the weight a tendency towards that ſide to which it deviates; and that ſometimes ſo ſuddenly, that without care, and much force applied, if the weight be very great, the burden will ſwing to or from the carriage; ſo as to break every thing in its way. Sometimes a horizontal piece, like a hand-ſpike, is fix'd in the upright ſhaft of the gibbet a little above B, to turn it by; but in that caſe too the force is unequal, as the weight is carried round; ſo that the lives of the men that are loading, often depend upon the care of the man who guides the weight, by either of the means above-mentioned.

N. B. No ſituation of the pullies can prevent this; and we find accidents to happen every day, as will appear by examining Fig. 5.

But if upon the axis of the gibbet there be fixt an iron wheel *y*, with ſeveral teeth, to be carried round by a pinion *u*, of a few leaves; upon the end of whoſe axis is faſtened a wheel *x*, with arms (that axis going thro' the perpendicular piece TX, behind the ſhaft of the gibbet) a man ſtanding at that wheel is out of harm's way; and has ſuch an advantage of power, as to hold the weight ſteady in any place required, notwithſtanding its tendency to ſwing, as mentioned above, which is not felt at the ends of the arms of this laſt wheel. The firſt who made uſe of this contrivance was Mr. *Ralph Allen* of *Bath*, at his ſtone-quarry, where the weight rais'd is 4 or 5, and ſometimes 6 or 7 ton.

It need not be ſaid, that the power to bring up the weight works here by means of a capſtane, or upright ſhaft, RO, drawn round by horſes, that the weight may come up more expeditiouſly; tho' the hand-ſpikes f, e, b, going in at ſuch a hole as d, as in the figure, ſhew that men may work it on occaſion.

The ſame gentleman having laid his ſtone on waggons of a peculiar make, cauſes it to run down hill about a mile and a half on a wooden waggon-way to the river ſide, where he has a wharf, and there by another crane exactly ſuited to the work, he takes the ſtone from the carriages; and with great expedition lets it down into the barges or veſſels, that come to fetch it.

The crane is of the ſort commonly call'd a rat's-tail-crane, moving round a ſtrong poſt like a wind-mill; ſo that it may turn quite round with all its load: The axle B b (Fig. 9.) on which the rope winds is here horizontal like a winch; but to gain

gain strength, instead of the walking wheel C A, it is carried round by a strong wheel and pinion (Fig. 7, 8.) or is in effect a double *axis in peritrochio*. Now in the common cranes of this kind, there is only a catch as E K A (Fig. 7.) to hold the burden to the height it is brought up to; whilst the crane is turned round, in order to have the weight lowered into the vessels; which is done by lifting up the catch, and being ready to let it down again as need requires. Sometimes a half circumference of wood D I I B (Fig. 7.) is held hard against a wooden wheel W *w*, on the axle, to regulate the descent of the weight. But as in either of these cases, if the man at the crane be careless, very bad accidents happen; Mr. *Padmore*, Mr. *Allen*'s chief workman, made such a contrivance, that the pall or lever, by which the axle is press'd to direct the descending motion, communicates with the catch in such a manner, that in case the man who ought to manage it, should carelessly let it go, the catch always takes, and thereby all accidents are prevented; as shall be shewn in explaining Fig. 7, 8.

Where goods are to be rais'd high, as in unloading vessels, and likewise to be let down deep, as in loading them (that is, where both the former operations are to be performed) if the weights do not exceed two or three ton, and several hands are not to be had; then an endless screw turn'd by a handle at each end (in an opposite situation, or with one handle and a balance to it) leading an axis *in peritrochio*, or as it is commonly call'd, a worm and wheel applied to a crane, with a gibbet, is most useful: For, the teeth of the wheel are pull'd by the weight so directly against the thread of the worm in its endeavour to descend, that one may leave the handle in any position where it will stop without any catch, or the least danger of the weight falling back again.

But then, if you would have the weight to be let down, to descend pretty quick, which cannot be perform'd by applying the hand to the handle, which goes thro' a large space, in comparison to the space, describ'd by the weight (without which sufficient force would be wanting) only give the handle a swing; and if the worm be well oil'd, the handle and its counterpoise, or the two handles, will perform the office of a fly in a common jack, turning round very fast, and regulating the motion of the weight, which from that impulse will descend continually, and not too fast, like the weight of a jack.

The way to stop this motion at any time is to grasp the axis of the screw hard between the screw and the handle in its round part, The hand is sufficient to do it, and it will stop it in 2 or 3 turns.

The worst cranes are those, where men walk in a large wheel, by reason of accidents that daily happen, on account of

the ſhort ſpace between a man's two feet: This may be prevented by uſing four footed animals, the length of whoſe bodies forms a baſe of ſufficient length to keep the wheel from running back.

Fig. 3. Plate XIII. repreſents a fixt crane with a gibbet moving on an upright ſhaft or axis; A *a* Q the roof of the crane to preſerve the rope R T *r* from the weather, when the arm of the gibbet V G *g* being turn'd towards Y is brought under it; A T the upper piece of the crane, call'd the plate of the crane, in a horizontal poſition; X Y Z the three crane-poſts, braced at top and bottom; DS, MN, I E three cills within the ſtone-work, braced with wood, and made faſt with an upright plate of iron, pinn'd to the wood on each ſide.

N. B. When the crane is not in ſtone-work, the three cills muſt be all in one piece, reaching from D to E.

H I, *b* E are the braces of the main-poſt of the crane, which come up above the level of the wharf L *w* B, which are longer and ſtronger than the others: Here a croſs piece whoſe ſection is (&) keeps the main-poſt from twiſting; R O the capſtane, or ſhaft of the crane to receive the rope or chain; which ſhaft is turn'd here by bars or hand-ſpikes, ſuch as *bd*, *fd*, or *ed*; the lower part being ſtrengthened with iron hoops above and below the holes at *d*, with a pivot or iron axis turning in a hole, in a piece whoſe ſection is F; *p p* are the two pins, which hold on a collar, in which the upper part of the ſhaft turns; C B the ſhaft or axle of the gibbet, with pivots and iron hoops at top and bottom, and a wheel of iron *y*, having teeth perpendicular to its plane. This wheel is led by a pinion *u*, which is on the axis of the wheel *x*, by whoſe arms a man ſtanding at H may bring about the end of the gibbet *g* with the ram-head *r*, and the weight hanging at it, either to the right or left, and eaſily hold the gibbet in any poſition; C T P Q a ſtrong piece or block having 3 pullies; one vertical, and the other two horizontal, that the rope may run over the firſt of them, and between the other two.

Fig. 4. repreſents a horizontal ſection of the crane in its upper part, or rather a view of it from the plane of the roof; ſuppoſing the roof to be taken off.

N. B. The ſame letters mark the parts, which have been deſcrib'd in Fig. 3.

Fig. 5. ſhews the inconveniencies in the motion of the gibbet; L B E D repreſents part of the wharf next the water, or precipice of a quarry: T P Q the block-piece which holds the 3 pullies, expreſs'd by the ſame letters in Fig. 3, 4; *ſ g r* G the arm of the gibbet, repreſented by V g, Fig. 3. T the vertical pulley; P, Q, the horizontal pullies, repreſented in another ſituation by p, q; when their centers from m, y, are brought

brought to n and t; C is a point directly over the pivot of the shaft, or axle of the gibbet; C 1, C 2, C 3, C 4, C 5, represents a line over the arm of the gibbet, or rather a place going thro' the middle of it, in several of its situations, when turn'd towards the right hand, from its direct position C r; C 6, C 7, C 8, C c, represent the several situations of the gibbet towards the left; the last pulley *r*, at the end of the gibbet, immediately over the weight traversing in the circle 5, 4, 3, 2, 1, 6, 7, 8.

When the gibbet is in the position *cg* the rope runs directly over the middle of its arm; therefore, the least force, applied to r or *r*, can keep in its place the greatest weight that can be drawn up by the crane, when suspended to the ram-head. If the pullies are at *p* and *q*, the gibbet loaded will also be without labour retained in the position C 2 on the right, and C 6 on the left; and with no great trouble in the position C 1.

But if the gibbet be brought over the wharf at 4 on the right, or at 8 on the left, the rope will no longer run over the middle of the gibbet, but deviate from it; so as to form with it the angle q 4 t, or o 8 n, and raise the weight by the motion of the gibbet, in proportion as the line q 4, o 8, is longer than t 4, or n 8: and therefore, the weight will tend to run back towards g in proportion to the difference of those lines, which must give a twitch to the person who draws from r, or *r* by a guide-rope.

If to prevent this inconveniency, the pulley at q be removed back to Q; then it is true, the rope will run over the line C 4, or t 4; and consequently, the gibbet will be easily held in that situation; but if you have occasion to move the weight to 5, the rope touching the pulley at t, will form an angle with C 5, and again be subject to the inconveniency above-mentioned: Besides, in bringing the end of the gibbet from g to 4, the rope immediately applying itself to the pulley at t will come forward with a jerk; tho' it will be twitch'd back again when at 5.

If the pulley be set still more backward, as may be seen at P, when you would keep the weight under 8, it will tend to go on towards c, in proportion as the rope at m 8 is now shorter than the line n 8; for, now the weight descending a little, the force of that descent, added to the pull of him, who draws the guide rope, will cause the weight to swing towards the crane; so as sometimes to do mischief, if the weight be very great, and the man careless.

N. B. No position of the pullies can mend the matter, there being only three situations of the gibbet in its whole traverse, where it can keep its place when loaded: Therefore the wheel y, and the wheel and pinion x u, in Fig. 3. are of very considerable use, when great weights are raised.

Fig. 6. represents the double *axis in peritrochio*, or wheel and pinion used instead of the walking wheel of Fig. 9. c, c an

 axis

axis with handles, having a pinion P, which leads the wheel P R to wind the rope R Z on the axle R; K A part of the catch, which ſtops the rope from running back again; W, w, a wooden wheel of ſome thickneſs, which (when the catch is up) is kept from turning too ſwift, as the weight runs down, by pulling up the ſemicircular part of the pall I o I, ſo as to make it bear hard againſt the wheel below, to regulate or ſtop the deſcent of the weight; C C the pivots or centres of the axle; L F part of the lever, by which the pall is drawn up againſt the wheel W w, by means of the rope F B; Q the weight to bring down the pall clear of the wheel W w, when it is not pull'd up; I o I B the end of the pall which is applied to the wheel, the other end not being repreſented here.

Fig. 7. Shews the manner of letting down the weight ſwifter or ſlower; as there is occaſion, repreſenting that end of the axle on which the catch and pall act alternately; P P and p p are two upright pieces fixt to the frame of the crane, in any manner that is moſt convenient for carrying the three centers L, K and k; when the rope R r Z going over a pulley at r, or any where elſe, draws from the axle in the direction R r; the catch, if its end is at A, keeps it immoveable; but by pulling at H, the lever G F riſes at F, and conſequently draws up the end B of the pall B D; which moving on the center k, does by its end D (by means of the bar D E) pull down E, and raiſe A of the catch; ſo as to let the rope run down: But to prevent its running too faſt, one muſt pull a little harder; then the ſemicircle I o I will preſs againſt the wheel, and ſlacken the deſcent of the weight, which will be entirely ſtopp'd by pulling ſtill harder: Then the lever, pall, and catch, will be in the poſition mark'd by prickt lines, and ſmall letters. Now if the perſon, holding H, ſhould careleſſly let it go, the weight Q in deſcending will bring down the pall at B, and raiſe its other end; ſo as to throw the catch in again upon the teeth of the rachet, and ſtop the whole motion without accidents.

Fig. 8. repreſents the wheel and pinion at the other end of the axis, where the ſame letters expreſs the ſame parts.

Fig. 9. repreſents the crane with the walking wheel; the whole turning round upon the ſtrong poſt or puncheon S, which is fixt ſteadily upright by means of the braces and cills L L L L L L L L; and when the wheel and pinion is uſed inſtead of the walking wheel, all the other parts are the ſame; f F is a brace and ladder; E, N, M, F, pullies for the rope to run over, and come to the weight at H.

N. B. Sometimes a pair of blocks is applied between F and H: A ſmall wooden roof is likewiſe applied over the ends of the pieces at E, N, M, and F.

The END of the EIGHTH VOLUME.

THE

THE

INDEX.

Elephants

Misselto,

Tunis,

CORRIGENDA.

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